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TOPICAL DISCUSSION ON THE RÔLE OF THE SEVERAL ALLOYING ELEMENTS IN THE ALLOY STEELS.

THE RÔLE OF MANGANESE.

By Henry M. Howe.

Passing by the deoxidizing and desulfurizing effect of manganese as foreign to our present purpose, its effect on the mechanical properties of the steel seems to me in the last analysis due primarily to its retarding action both on the transformations and on the coalescence of the micro-constituents into progressively coarser masses, which while increasing the ductility lessens the cohesion in general, including the hardness and the elastic limit, and thus lessens the effective strength.

We may consider the action of manganese first in Hadfield's manganese steel, containing about 12 per cent of manganese, and second in carbon steel, in which the proportion of manganese rarely exceeds 1.50 per cent.

Before considering the retarding of the transformations by manganese let us refresh our memory as to these transformations, and as to the three prominent states of steel, between which they play, (1) the common low-temperature alpha or pearlitic state, (2) the high-temperature or non-magnetic austenite state into which the metal passes spontaneously when heated up through the transformation range, say 725 to 900° C., Ac1-Ac3, and (3) the intermediate or martensitic state, in which carbon steel is caught in transit from the austenite to the pearlite state by means of a rapid cooling, as for instance on hardening by quenching small pieces in water. The alpha state is magnetic and relatively soft and ductile, as in annealed carbon steel;

the intermediate or martensitic state is magnetic, hard, and brittle as in hardened steel; while the non-magnetic high temperature or austenitic state when preserved in the cold, as in manganese steel, combines great ductility with hardness of a peculiar kind to which I will refer shortly.

In carbon steel this transformation is so rapid that it occurs to a very marked degree even in the water quenching of thin pieces, as is familiar to us in the fact that when this steel is made non-magnetic and austentic by heating say to 900° C., and is then quenched in water, it transforms as far as the magnetic, hard, brittle, martensitic state of common hardened steel even in this rapid cooling.

Most of the alloying elements, and notably carbon, manganese, and nickel, retard this transformation greatly. Thus 2 per cent of manganese plus 2 per cent of carbon retard it so that in the water quenching of thin pieces the austenite state is preserved. With 5 to 7 per cent of manganese it is so slow that even in air cooling it goes only as far as the intermediate martensitic state. Hence the brittleness of these steels of intermediate manganese content. With say 12 per cent of manganese the transformation is so sluggish that the austenitic state is preserved even through a common slow cooling. The water-quenching of manganese steel in current manufacture is not to prevent the loss of the austenitic state, but to suppress the precipitation of the iron-manganese carbide, cementite, which would occur during slow cooling. The broad plates of this cementite would embrittle the mass by forming partings of low cohesion. It is derived from the large carbon content of the ferro-manganese used, the cheapest source of manganese. Carbon-free manganese steel should not need quenching.

The industrial value of this manganiferous austenite or manganese steel seems to be due to its combination of great ductility with great effective hardness. I say effective hardness, because initially it is rather soft. My own experiments[1] indicate that the Brinell hardness of an undeformed specimen is only 125, or that of steel of about 0.22 per cent of carbon when annealed, that of ultra low-carbon steel being about 75. But the hardness increases very greatly on the slightest deformation. Even that incidental to the Brinell test increases the observed Brinell hardness to 223 easily, or to that of 0.60-per-cent carbon steel when annealed.

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[1] "The Metallography of Steel and Cast Iron," p. 464, 1916.

This hardening under deformation is one of the first things that forces itself on the user of this material. The first strokes of the hack saw cut it rather easily, but the deformation thus set up in the path of the saw quickly causes such hardness as to bring the sawing to an abrupt end, thus giving the absolutely false impression that the material has a soft skin. This hardening causes the apparently contradictory combination of effective hardness with very low proportional limit, even as low as 28,250 lb. per sq. in. The proportional limit represents the cohesion of the undeformed material, the effective hardness represents the cohesion as exaggerated by the deformation incidental to service. In the same way the act of tensile rupture may increase the Brinell hardness to 540,[1] or that of about 0.50-per-cent carbon steel when hardened.

The surface of the jaw of a manganese-steel rock crusher, deforming under the great pressure, quickly hardens itself, so that the combination of a very hard surface with a ductile back develops spontaneously. As fast as this hard surface wears away it is replaced by a new one made equally hard by the deformation which it at once receives.

This hardening probably represents in part the same cause which leads to the increase of cohesion in general, including the hardness, of all the malleable metals under all forms of deformation, such as wire drawing, and in part the martensitization of the austenite. That is to say, the arrested transformation from austenite through martensite to the alpha state which is due in cooling through the transformation range but is restrained by the retarding action of the manganese, is now stimulated by the deformation sufficiently to cause it to proceed as far as the martensitic state, with consequent hardening and embrittling effect. This martensitization through the stimulation of the arrested transformation by deformation is a common property of austenitic steels which have only a moderate excess of the retarding elements over the quantity needed for causing the retention of the austenitic state. It occurs strikingly in austenitic 20-per-cent nickel steel.[1]

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[1] Hadfield and Hopkinson, *Transactions*, Am. Inst. Min. Engrs., Vol. 50, p. 486 (1914), and *Journal*, Iron and Steel Institute, Vol. 89, I, pp. 112 and 124 (1914).

Turning now to carbon steel, the retarding effect of manganese on the structural changes shows itself by leading in general to finer structure,[2] to finer ferrite masses, finer network structure, and finer pearlite, indeed probably often to the replacing of lamellar pearlite with sorbite. This greater fineness leads to better quality in general, and to a higher elastic limit in particular, though of course with a corresponding sacrifice of ductility. The great value of manganese for this purpose has not begun to receive the attention which it deserves. It is probable that a manganese content of say 1.25 per cent, with a correspondingly lessened carbon content, may be used so as to lessen the danger of cracking and the residual stresses when a high elastic limit is sought, because this large manganese content in and by itself raises the elastic limit by giving a fineness of structure which otherwise would be sought by increased violence of cooling or by the use of a lower drawing temperature. In other words, the use of 1.25 per cent of manganese lessens the needed violence of cooling, and permits the use of a higher residual drawing temperature, in both ways tending to mitigate the stresses, and in the former way lessening the chances of cracking.

An additional way in which manganese retards and lessens the rapidity of cooling needed to cause hardening is its retarding the transformation itself.

The lessening of the rapidity of cooling to which I have referred may be brought about in various ways, for instance by substituting oil for water quenching, or air cooling for oil quenching, or quenching in water covered with a film of oil, or withdrawing the quenched object from the quenching bath after a predetermined time.

What I have said is wholly consistent with the tendency of manganese to increase the danger of cracking for given rapidity of cooling and for given carbon content. It is by enabling us to lessen the carbon content, or the rapidity of cooling, or both, that manganese may be made to lessen the danger of cracking.

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[1] Carpenter, Hadfield, and Longmuir, Seventh Report, Alloys Research Committee, (British) Inst. Mech. Eng., 1905, p. 949.

[2] Henry M. Howe, "Life History of Network and Ferrite Grains in Carbon Steel," *Proceedings*, Am. Soc. Test. Mats., Vol. XI, pp. 322 and 365 (1911). Howe and Levy, *Proceedings*, Cleveland Inst. Engineers, July 1914, p. 237.

THE RÔLE OF NICKEL.

By R. R. Abbott.

In its most simple and fundamental form steel is a more or less rigid mechanical mixture of pearlite and iron, the pearlite itself being a much finer mechanical mixture of cementite (Fe_3C) and iron. In common with such mechanical mixtures the physical properties of this alloy bear some ratio to the percentage of its constituents. For various reasons the ratios of these physical properties are not straight-line functions of the carbon contents. One of these reasons is that fact that, practically, we do not have a perfect mechanical mixture, but the iron always carries some carbide in solution, the amount of which increases with the carbon content of the alloy. Another reason is the changing ratio between the areas of the boundaries between the iron and the cementite, and the amount of these two constituents as the carbon content varies.

Fig. 1 represents relations existing between the carbon content and the elastic limit, tensile strength, reduction of area and preçentage of elongation of pure iron-carbon alloys up to 0.9 per cent of carbon (approximately from 0 to 100 per cent of pearlite). Fig. 1 does not represent ordinary steels, as the influence of nothing but carbon is considered. For the sake of simplicity and because it represents very accurately true conditions, it has been drawn as two lines with the break at 0.45 per cent of carbon. It also represents perfectly annealed alloys. Cementite is extremely hard, strong and brittle: iron is soft and ductile: therefore we should expect to obtain with any single alloy, physical properties depending upon the percentage composition of the constituents.

Now it is very evident that in order to make a change in the physical properties of any individual alloy of this series, we could proceed in one of three ways: (1) To change the nature of the pearlite; (2) to change the nature of the iron; and (3) to change the nature of both the pearlite and the iron. The second cannot be entirely distinct from the first procedure, because of the iron in the pearlite.

In practice the physical properties of this simple iron-pearlite alloy is changed by three methods:

1. Addition of various elements to it;
2. Changing the relative amount of free iron present; and
3. A combination of both methods Nos. 1 and 2.

Under method No. 1, if more than the normal amount of any element present in a simple carbon steel is added, the steel is known as an alloy steel. Under method No. 2 come the various processes of heat treatment.

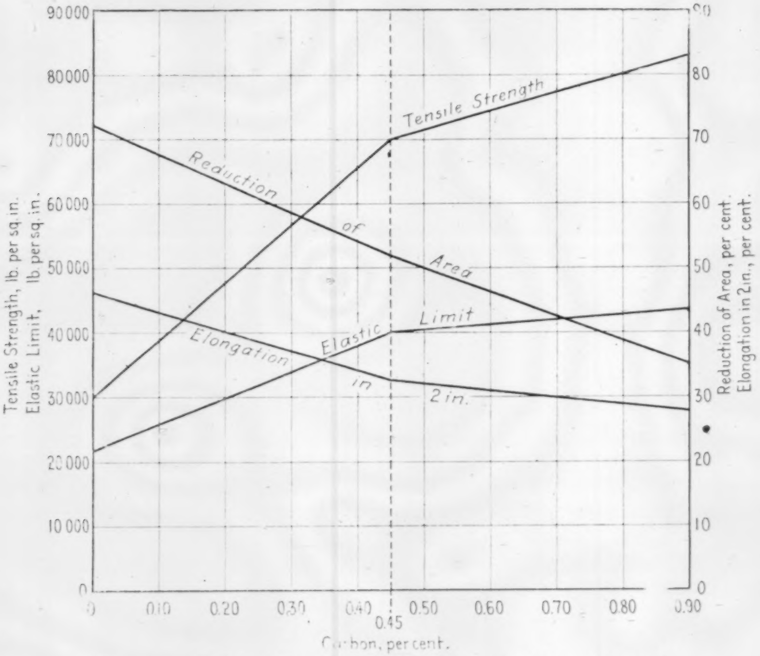

FIG. 1.—Physical Properties of Pure Carbon-Iron Alloys.

When nickel is added to our iron-pearlite alloy it dissolves entirely in the iron, and the physical properties of the alloy are changed approximately to the extent we should expect, when we consider that it is a solid solution of nickel and iron for one element and a pearlite with this solution for one of its constituents, for the other.

The effect of alloying nickel with pure iron is to increase the elastic limit, tensile strength, and reduction of area, but to decrease the percentage of elongation. This effect is the same when carbon is present, and it is found that a given amount of nickel produces about the same change in physical properties, irrespective of the percentage of carbon.

The average effect of nickel up to 8 per cent, upon an iron-carbon alloy is as follows:

0.01 per cent of nickel increases elastic limit..... 40 lb. per sq. in.
0.01 " " " " tensile strength. 42 " "
0.01 " " " " reduction of area 0.005 per cent.
0.01 " " " decreases elongation..... 0.010 " "

Expressing this for a 3.5-per-cent nickel steel, which is the most common commercial alloy, we see that such a nickel steel should have 14,000 lb. per sq. in. higher elastic limit, 14,700 lb. per sq. in. higher tensile strength, 1.5 per cent greater reduction of area, 3.5 per cent less percentage of elongation than a steel of the same analysis, but without the nickel. (By the percentage of reduction of area and elongation, is meant the actual number of units of change. For example: A steel with no nickel has a reduction of area of 53 per cent and an elongation of 26 per cent; the same steel with 3.5 per cent nickel would have a reduction of area of 54.5 per cent and an elongation of 22.5 per cent.)

The elastic limit and tensile strength are then increased by nickel due to the increased properties of the solid solution of the nickel in the iron. The reduction is practically altered but little, but the elongation is quite appreciably decreased.

When a plain carbon-iron alloy is heated above the upper critical point (Ac3), the cementite is dissolved in the iron of the pearlite at the passage through the lower critical point (Ac1) and this solution progressively dissolves the remaining iron during the heating up to the Ac3 point, at which point it is all dissolved and the solution is homogeneous. Upon cooling, the reverse action takes place, but when nickel is present this separation of the cementite from the iron-nickel solution does not progress as completely nor as easily as from a plain iron solution, and therefore the pearlite areas in a nickel steel are larger and much poorer defined than would be the case in the same steel without nickel. This effect becomes more marked the more rapid the cooling, and since the pearlite areas are much stronger than the iron areas we find that in practice a nickel steel, compared to one without nickel, when air cooled, will have a higher increase in tensile strength than that given above, which represents prefectly annealed alloy.

When steel is broken down too rapidly in rolling, it frequently shows a decided segregation of the pearlite areas along lines parallel to the rolling direction. Nickel apparently tends to intensify this effect and occasionally under even normal rolling this condition is encountered in nickel steel.

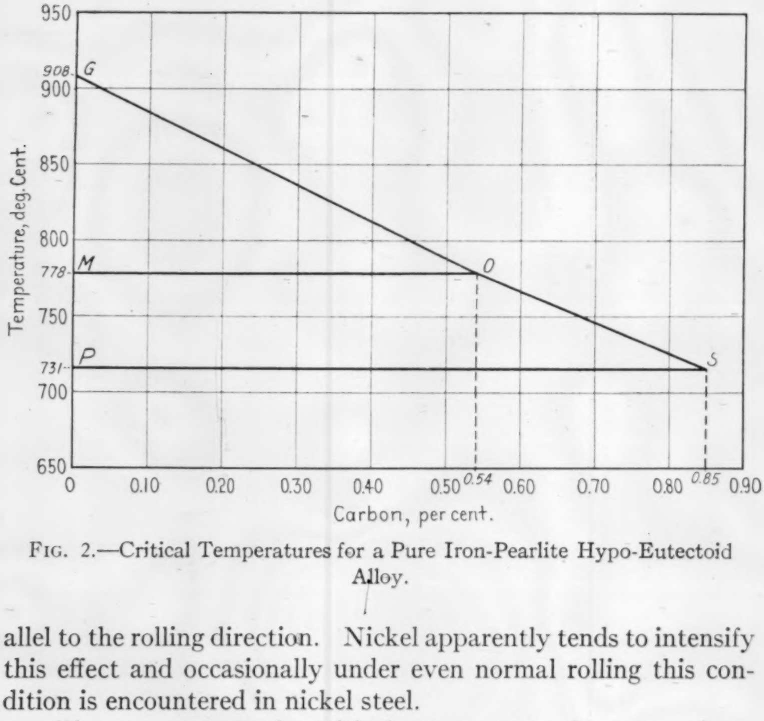

FIG. 2.—Critical Temperatures for a Pure Iron-Pearlite Hypo-Eutectoid Alloy.

Fig. 2 represents the critical temperatures for a pure iron-pearlite hypo-eutectoid alloy upon heating. Line *GOS* represents the upper absorption point (Ac3), *MO* the magnetic point (Ac2), and *PS* the lower absorption point (Ac1).

The lower and upper absorption (critical) points upon heating, as well as the magnetic critical point, are all lowered by the addition of nickel. The amount of the lowering is given approximately by the following:

0.01 per cent of nickel lowers *GO*.................. 0°.235 C.
0.01 " " " " *OS*.................. 0°.180 C.
0.01 " " " " *MO*.......... about 0°.087 C.
0.01 " " " " *PS*.................. 0°.103 C.

In working with the Ac2 point these figures are not based upon as accurate an investigation as for the Ac1 and Ac3 points. As the nickel contents are increased and the critical temperature progressively lowered, it is evident that finally the upper absorption point will reach the temperature of the atmosphere (20° C.) and we will have an austenitic alloy; the greater the amount of carbon the less nickel it takes to reach this condition. Also as the nickel, increases points *O* and *S* (Fig. 2), move to the left until they finally successively coincide with *G*. As soon as the line *MO* or any point on *OS* are brought below the temperature of the atmosphere, we have a non-magnetic alloy.

When a piece of steel is quenched from above the upper absorption point rapidly enough the result will be the complete absence of iron as a constituent of the alloy. This process is the basis of most heat treatments.

The martensite which is thus produced has physical properties entirely different from the mechanical mixture with which we started. A martensite containing nickel has a higher elastic limit and maximum strength, but a slightly lower reduction and elongation than a similar one without the nickel.

A heat-treated nickel steel, while having a lower reduction and elongation than a correspondingly heat-treated steel without nickel, has more than enough increase in strength to make up for it; in other words with a given strength the nickel steel will have a greater degree of toughness. Likewise, as we increase the strength of an alloy (either heat-treated or not) by means of nickel, the loss in reduction of area and percentage elongation is far below the loss secured when the strength is increased by increasing the carbon content. In other words nickel as a strength-giving element is not accompanied by such a loss in toughness as is carbon.

The effect of nickel upon the elastic limit and tensile strength of heat-treated steel varies with the amount of "draw" or temperature of reheat after quenching.

The following formulas represent the effect of 0.01 per cent of nickel upon a $\frac{3}{4}$-in. bar of steel; in these formulas T is the temperature of draw in degrees Centigrade and the results represent the increase in pounds per square inch.:

Elastic limit	$287 - 0.174\,T$
Tensile strength	$294 - 0.214\,T$

The reduction of area and percentage of elongation are reduced approximately the same amount, and this amount is not affected greatly by the temperature of draw; 0.01 per cent of nickel lowers the reduction of area and percentage of elongation about 0.0073 per cent in heated-treated steels. This slight amount is practically negligible for commercial nickel steel.

Nickel decreases the rate at which steel will absorb carbon in the carbonizing process. The amount of this is approximately 0.04 per cent for each 0.01 per cent of nickel.

THE RÔLE OF SILICON.

By W. E. Ruder.

For many years after Mushet found that quartz sand, when added to molten iron, produced a product that was hard and brittle, the presence of silicon in steel was looked upon with suspicion and distrust. Even to-day there are undoubtedly many metallurgists who would be very slow to specify even small percentages of this element for most grades of steel.

Less than thirty years ago, however, the researches of Barrett, Brown and Hadfield[1] gave to silicon a place as a useful alloying element with iron, which has not since been usurped or even approached by any other element. It is true that in this country, some years before this, small percentages of silicon had been found useful to reduce magnetic aging of mild steels used in the construction of magnetic circuits, but the percentage was so small that it could scarcely be classed as an alloy steel.

The effect of small percentages of silicon, such as are met with in carefully made steel from any process, upon the mechanical properties has been carefully investigated by many authorities and it is the general concert of opinion that its effect is negligible.

Where it has been found in proportions exceeding 0.1 per cent and poor results obtained, such results are usually traceable to accident or lack of care in manipulation during the process of manufacture.

It is beyond the scope of a discussion of this kind to lay before you again the details of any of the very valuable experiments carried on by Turner[2], Baker[3], Hadfield, Paglianti[4], Charpy and Cornu-Thenard[5], Gumlich[6] or Guillet[7]. I desire only to outline the essential points which these investigators have established and to give opportunity thereby for a discussion of their merits.

---

[1] Barrett, Brown and Hadfield. *Scientific Transactions*, Royal Dublin Soc., Vol. 2, Part 4 (1900); also *Journal*, Inst. Elec. Engrs., Vol. 31, p. 674 (1901).

[2] Report of Committee British Association, 1888.

[3] Baker, *Journal*, Iron and Steel Inst., Vol. 64, No. II, p. 312 (1903).

[4] Paglianti, *Metallurgie*, Vol. 9, p. 217 (1912).

[5] Charpy and Cornu-Thenard, *Journal*, Iron and Steel Inst., Vol. 91, No. I, p. 276 (1915).

[6] Gumlich, *Transactions*, Faraday Soc., Vol. 8, p. 98 (1912).

[7] Guillet, *Revue de Metallurgie*, Memoirs, Vol. I, p. 46 (1904).

Effect of Silicon upon Mechanical Properties.—In most of the investigations the effect of silicon, particularly in amounts less than 1 per cent, have been masked or modified by the presence of varying percentages of manganese and carbon. In a recent paper Yensen[1] has re-examined the series of forgeable silicon alloys using as a base pure electrolytic iron and giving us a more exact idea of the effect of silicon alone. Although his figures differ in some cases from those found by previous investigators the general trend of his curves is the same.

Silicon increases the tensile strength of pure iron from 38,000 to about 90,000 lb. per sq. in. at 4 per cent in annealed samples. Beyond this point the value rapidly falls off. The elastic limit is likewise steadily increased up to 4 per cent silicon. The ultimate elongation and reduction of area are practically unaffected until 2.5 per cent of silicon is reached. From this point the drop is rapid.

It is interesting to note that whereas Yensen's values for elastic limit and ultimate strength are generally lower than those of previous investigators, his values for reduction of area and ultimate elongation are in general higher, particularly for silicon content below 2.5 per cent. This would indicate that silicon in itself does not cause brittleness when below 2.5 per cent, and that the presence of small percentages of carbon increase the strength at some sacrifice of ductility. The decrease in ductility is about coincident with the beginning of the marked tendency for large grain growth which silicon imparts to mild steel and with the beginning of the precipitation of graphite.

Although the limit of forgeability is given as about 7 per cent of silicon, the practical limit for sheet rolling is something under 5 per cent.

An interesting new development in the study of iron-silicon alloys is noted in Yensen's work in which he appears to have established a heretofore unnoticed critical point at which there is a sudden drop in all mechanical values at about 2.5 per cent of silicon. The phenomenon was first noticed from the fact that two ingots in this range broke up in forging. A further examination showed that there was a sharp drop in all values at 2.5 per cent and an equally sharp recovery at 2.7 per cent of silicon. The formation of a compound $Fe_{19}Si$ is suggested as an explanation, but seems hardly adequate. However, no satisfactory explanation has yet been arrived at.

---

[1] Yensen, *Bulletin No. 83*, University of Illinois, November, 1915.

Effect of Silicon Upon the Critical Points.—Gumlich has shown that the second (magnetic) transition point is steadily lowered with increased silicon content until it reaches a value of about 660° C. for 8.35 per cent silicon, while the first (pearlitic) point steadily rises, the two coinciding at 2.2 per cent silicon, and becomes practically extinct at 2.5 per cent silicon. In an exhaustive study Charpy and Cornu-Thenard later confirmed this observation and definitely connected the disappearance of the A1 point with the precipitation of graphite.

Silicon forms a solid solution with iron in all proportions up to about 20 per cent of silicon and since this is far beyond the region of present usefulness it is unnecessary to discuss, at this time, the iron-silicon diagram.

Effect of Silicon Upon Magnetic Properties.—Just as tungsten, chromium, nickel and manganese have found their specific uses when alloyed with iron, so also silicon quickly found its place as an alloy for magnetic circuits for alternating fields. Although carbon steels, containing from 0.2 to 0.4 per cent of silicon, are made for tires and automobile springs, and certain tool and gun steels are reported to have from 0.3 to 0.6 per cent of silicon, the almost exclusive use of silicon alloys is for magnetic circuits. Something over 100,000 tons of silicon alloy sheets were used in this country alone during the past year.

Silicon raises the resistivity of mild steels about 10 microhms per cubic centimeter for each per cent added, so that a silicon steel containing 4.5 per cent of silicon has a resistivity of 59 microhms per cubic centimeter. This has an important effect in reducing the eddy current losses.

Gumlich found that the coercive force was very greatly reduced by the addition of 0.06 per cent of silicon in both rods and sheets. This reduction continued in the sheets, reaching its lowest value at 0.4 per cent of silicon and thereafter rising. In rods, however, there is an immediate rise up to 1 per cent of silicon, and then a gradual falling off. Yensen, on the other hand, working with very pure alloys in the shape of rods only, finds two minimum points of equal value, one at 0.15 per cent silicon and one at about 4 per cent. In maximum permeability there is fair agreement as to the shape of the curve, that is, each shows two maximum values at about 0.2 and 3.5 per cent respectively, although their numerical values differ very greatly. In this case, however, we are comparing Yensen's rods with Gumlich's sheets. From his rods the latter gets widely different and unexplained results.

The explanation of the wide difference which Gumlich finds between sheets and rods and the agreement in the shape of his curves for sheets with those of Yensen's rods probably lies in the fact that the former's sample contained from 0.18 to 0.34 per cent of carbon. This carbon remained in the combined form for the rods under 2 per cent silicon, while in the sheets it precipitated out as graphite, this graphitization being aided in this case by the freer access of oxygen during the annealing and possibly by the fact that it had more severe mechanical working.

The effect of silicon, therefore, seems to be, unlike that of the ferromagnetic metal cobalt, an indirect one. Its first effect is to act as a deoxidizer, its presence in small percentages indicating its excess over the amount of removable oxygen originally present, and therefore explaining the first peak in permeability curve and depression in hysteresis value. From this point on to about 2 to 2.5 per cent, the silicon merely acts as a diluent causing a steady drop in saturation value and permeability and an increase in coercive force. At about 2 per cent, however, its influence as a grain growth[1] stimulator becomes evident (Hadfield) and there is a consequent rise in permeability and decrease in coercive force. This effect reaches a maximum and from this point onward any added silicon again acts as a mere diluent.

In some recent unfinished experiments I have gathered some evidence that hydrogen appears to form an alloy with iron producing much the same effect on the maximum permeability and saturation value. This alloy, however, appears to be stable only at low temperatures.

The increased solvent power for gases imparted by the silicon and the decreased solvent power for carbon are also undoubtedly large factors where these are present. In commercial silicon steels these properties are of the utmost importance. Charpy and Cornu-Thenard have shown that a 3.9-per-cent silicon steel containing 0.14 per cent of carbon had its carbon completely separated out as graphite after an annealing of three hours at 800° C. This graphitization accounts in part for the second maximum point, particularly in alloys containing over 0.1 per cent of carbon.

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[1] Ruder, *Bulletin*, Am. Inst. Min. Engrs., December, 1913, p. 2805.

The effect of grain size on the two components of total watt loss, namely, hysteresis and eddy currents, and upon the permeability, is interesting and the reason is not altogether clear. Increased grain size causes a decrease in hysteresis and an increase in permeability at medium and low densities. The eddy current losses increase with the grain size but apparently not in as direct a ratio as the decrease in hysteresis. From recent data, this increased permeability seems to be due to a higher permeability in the direction of crystal orientation and the high eddy loss to a consequent non-uniform permeability in the cross-section tested.

Heretofore it has been considered that the resistivity of the material and the thickness of laminations was the sole factor governing the eddy current losses, but I have found many cases[1] where the eddy current losses have been doubled with increased grain size without change in resistivity or thickness in lamination.

Increased grain size in the direction of rolling also accounts for the increased permeability and decrease in watt loss of silicon steel[2] when tested in sheets cut parallel to the direction of rolling when compared with those cut across.

Other elements as tin and arsenic have been shown (Burgess & Aston) to have similar effects upon magnetic properties of iron, but none of them combine all of the beneficial effects of deoxidation, graphitization, increased grain size, increased electrical resistance and solvent power imparted by silicon.

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[1] Ball and Ruder, *General Electric Review*, Vol. 17, p. 487 (1914).

[2] Spooner, *Electric Journal*, Vol. 14, p. 90 (1917).

THE RÔLE OF VANADIUM.

BY G. L. NORRIS.

The metallic element vanadium is one of the fifth group of chemical elements which includes phosphorus, arsenic, antimony, nitrogen, etc. It has an atomic weight of 51.06; specific gravity of 5.5, and melts at 1680° C. It is grayish-white in color, non-magnetic, has high electric resistivity, is the hardest of the metallic elements, and the most difficult to reduce. It has not been produced in the pure metallic state. Although discovered in 1830 and soon found to be widely distributed, its known occurrences were in such small quantities that for many years vanadium was considered one of the rarest elements.

The first metallurgical application of vanadium appears to have been in 1896, when it was used in making several heats of steel at Firminy, France. The results of this test showed a remarkable superiority of the vanadium steel in physical properties. Under the same heat treatment, the vanadium steel showed an increase of 44.8 per cent in elastic limit and 31.5 per cent in tensile strength, with slightly lower percentage of elongation but greater reduction of area.

These tests developed considerable interest in the metallurgical possibilities of vanadium, and the manufacture of ferrovanadium was actively undertaken, notably by The New Vanadium Alloys Co., Ltd., of London, from the vanadium lead ores of Santa Marta, Spain. It was at the instance of this company that Professor Arnold of Sheffield undertook, in 1899, investigations of the effect of vanadium on steel. These investigations demonstrated conclusively that the presence of a few tenths of a per cent of vanadium gave a remarkable increase in elastic limit and tensile strength without seriously impairing the ductility, or presenting any difficulty in the hot or cold working. Professor Arnold at the time gave it as his opinion that vanadium was undoubtedly the element, which, together with carbon, acts with the greatest intensity in the way of improving alloys of iron; that is to say, in very small percentages.

Following the publication of these investigations, came in rapid succession those of J. A. Mathews and F. W. Taylor on high-speed steel; Sankey and Smith on chrome-vanadium steel; Paul Putz, Leon Guillet and others, so that by 1906 the metallurgy of vanadium as related to steel was well established.

At about the same time, 1905, the opportune discovery of an enormous deposit in the Peruvian Andes of a previously unknown mineral of vanadium, made it available commercially in quantities adequate to meet indefinitely all the requirements of the steel industry.

The following discussion will be confined almost entirely to the consideration of pearlitic-vanadium-ternary steel, or simple carbon-vanadium steel, and will not more than briefly touch on the quaternary-vanadium steels. First, however, the writer wishes to touch briefly upon a still somewhat prevalent popular idea that vanadium is a powerful scavenger, and that the beneficial effects of its use in steel are principally due to its removal of minute, residual amounts of oxygen and nitrogen from the steel. It has even been advanced by some individuals that when all the vanadium added has been completedly used up in scavenging, and none remains in the steel, all the improvement or beneficial effects possible have been accomplished; despite the fact that there is an increase in the mechanical properties of the steel with increasing amounts of vanadium present, and that vanadium has equally as great beneficial effects in steels made under reducing conditions such as crucible and electric furnace steels, as in the case of steels made under oxidizing conditions like open-hearth and Bessemer steels. While it is true that vanadium oxidizes readily and will combine with nitrogen, its value as a scavenger is negligible, as there are much cheaper metals that are as effective, or perhaps more so.

The remarkable effects of vanadium on steel are due entirely to its presence in the steel as an alloying element, and its influence on the other constitutents with which it is in combination. When added to steel it is found in both the main constituents, ferrite and pearlite, but principally in the latter. Only a few hundredths of one per cent of the vanadium combines with the ferrite. This minute amount, however, appears to increase the strength, toughness, hardness and resistance to abrasion of the ferrite. Nearly all the vanadium, however, is found in the pearlite, in chemical combination with the cementite, as a compound carbide of vanadium and iron in the case of ternary steel, and as more complex carbides in the case of quaternary steels.

Vanadium replaces the iron in the cementite or the carbide by increasing amounts until finally when the percentage of vanadium is about 5 per cent all the iron has been replaced by the vanadium. The vanadium containing cementite is not as mobile as ordinary cementite and consequently does not segregate into as large masses, but occurs in relatively minute particles and, therefore, is more uniformly distributed. It does not, consequently, readily occur as lamella or thin plates in the pearlite, but in a granular or sorbitic condition. This strong tendency of vanadium to form sorbitic and even troostitic pearlite, is doubtless one of the reasons for the mechanical superiority of steels containing vanadium, not only statically but dynamically.

Vanadium carbide is not as readily soluble on heating as iron carbide, and consequently vanadium steel requires a higher temperature to dissolve the cementite and put the steel in the austenitic condition for quenching.

The presence of vanadium does not raise the Ac1 and Ar1 points more than about 10° C., and repeated heatings do not seem to lower the Ar1 point. The Ac2-3 and Ar2-3 points are raised somewhat more and continue to rise with increase in percentage of vanadium.

The effect of vanadium on the physical or mechanical properties of steel increases with the percentage of vanadium until about 1 per cent is present, after which there is a decrease, even in the case of quenched steels, and with 3 per cent or more of vanadium the steel is actually softened on quenching until unusually high temperatures are reached, say about 1300 to 1400° C.

Vanadium steel hardenite has a greater thermal stability, or power to withstand elevated temperatures without softening or breaking down or separation of cementite. This property is responsible for the great improvement in high-speed steels, through the addition of vanadium to stand up longer under the high temperatures developed at the point of the tool in taking heavy cuts at high speed. Percentages of vanadium as high as

3.50 have been successfully used in high-speed steel, and 1.50 to 2.50 per cent are not uncommon, although only a few years ago the percentage ranged from 0.30 to 0.75, and it was thought that the addition of over 1 per cent gave very little additional advantage. The improvement in high-speed steel through the use of vanadium has borne an almost direct relation to the percentage of vanadium present, and is considered to be from 60 to 100 per cent.

In the case of carbon-vanadium tool steel the use of vanadium has proved almost equally beneficial, although at present only about 0.2 per cent of vanadium is used in such steel. It has a wider quenching range,—that is, can be heated higher without injury,—hardens deeper, retains cutting edge longer, and is very much tougher and stronger. A bar of 1-per-cent carbon tool steel containing 0.25 per cent vanadium, quenched and drawn back at 400° C., will bend 90 deg. without failure whereas a similar steel without vanadium will bend only about 20 or possibly 30 deg. Comparative compression tests of such tool steels with like tempering or draw back gave on $1\frac{1}{4}$-in. cubes, 490,000 lb. for the vanadium steel and 278,000 lb. for the steel without vanadium. For battering tools, such as pneumatic chisels, setts, calking tools, rock drills, etc., vanadium tool steel possesses marked superiority on account of its combination of hardness, strength and toughness.

One of the principal applications of vanadium steel has been for steel castings, particularly for locomotive frames. The composition of the steel is the same as usual for such castings, excepting for 0.15 per cent or more vanadium. The addition of this small amount of vanadium increases the elastic limit of the annealed castings 25 to 30 per cent without lowering the ductility. The tensile strength is not increased proportionately in the case of thoroughly annealed castings, but is usually 10 to 15 per cent greater. The following averages of tests within the same ranges of composition are typical:

AVERAGE CHEMICAL COMPOSITION.

Carbon, per cent	0.25
Manganese, per cent	0.62
Silicon, per cent	0.27
Vanadium, per cent	0.18

PHYSICAL PROPERTIES.

	CARBON STEEL.	VANADIUM STEEL.
Elastic limit, lb. per sq. in.	36 495	48 210
Tensile strength, lb. per sq. in.	73 820	79 930
Elongation in 2 in., per cent	27.3	26.6
Reduction of area, per cent	46.0	48.1

For reasons apparent from what has been previously stated, vanadium-steel castings require a somewhat higher annealing temperature than ordinary steel castings. They are also more susceptible to hardening, and therefore should be cooled slowly in the annealing furnace until they have cooled below the Ar1 point. The annealing temperature should be about 875° C.

There is a growing tendency to heat treat, quench and temper steel castings for many purposes. Vanadium steel is much more suitable for heat-treatment than ordinary steel, as it hardens more on quenching and consequently much higher physical properties can be obtained. Even air cooling from the annealing temperature, followed by an annealing at a low temperature, greatly increases the elastic limit without affecting the ductility. One company by this treatment meets the following minimum physical requirements with vanadium steel of the average chemical composition previously given:

Elastic limit, lb. per sq. in.	65 000
Tensile strength, lb. per sq. in.	90 000
Elongation in 2 in., per cent	20
Reduction of area, per cent	40

Tests of quenched-and-tempered castings of this same composition show as follows, the quenching and draw-back temperatures being the same for both steels:

	CARBON STEEL.	VANADIUM STEEL.
Elastic limit, lb. per sq. in.	58 630	72 860
Tensile strength, lb. per sq. in.	90 630	95 630
Elongation in 2 in., per cent	25	26
Reduction of area, per cent	52	56

Water was the quenching medium.

For higher carbons, the advantage in favor of vanadium steel is even greater.

There is also doubtless a great future for vanadium-quaternary-steel castings, both annealed and heat-treated—particularly the latter—for, as in the case of forged or rolled quaternary steels, the improvement in the mechanical properties from the presence of vanadium would be much greater even than in the case of simple carbon-vanadium steel. The value of vanadium in simple carbon forging steels has been over-shadowed by the greater mechanical properties of the vanadium-quaternary steels, such as chrome-vanadium, yet they have mechanical properties equal to those of ordinary 3-per-cent nickel steel under like conditions. Excepting where the very high physical properties obtainable from quaternary steels are desired, carbon-vanadium steel can be used to advantage, especially for large forgings.

This steel presents fewer manufacturing difficulties than quaternary steels. It is less liable to losses from shrinkage cracks and checks in the ingot, and to heating and cooling strains in the forging and heat-treatment operations. It requires no more special care in handling than ordinary carbon steel, and is worked with equal facility.

Carbon-vanadium forging steel in the normalized condition, which may be described as annealed for grain refinement, has physical properties superior to those specified for heat-treated carbon-steel forgings by this Society. This simple treatment alone, therefore, gives physical properties sufficiently high for a great number of forgings that would otherwise have to be quenched and tempered.

A large field for annealed or normalized carbon-vanadium steel is its use for locomotive forgings. It is generally conceded that there is need of a steel of greater strength, not only to meet present conditions, but also to permit of reducing sections of reciprocating parts to obtain better counterbalancing. To meet this requirement, the railroads several years ago turned to heat-treated carbon and heat-treated alloy steels, notably chrome-vanadium. The use of heat-treated locomotive forgings has not proved altogether satisfactory for a number of reasons. One objection to the use of these forgings is the lack of heat-treating equipment in most railroad shops. This operates particularly in repair work where for any reason the forging has to be locally heated to straighten, stretch or shorten, destroying thereby the

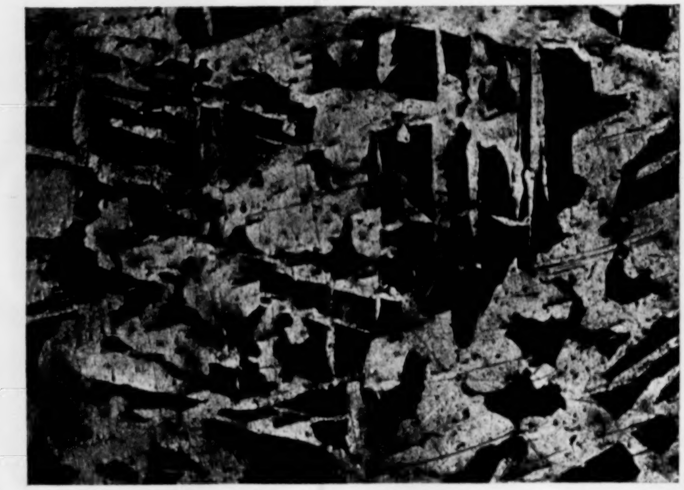

Fig. 1.—As Cast.

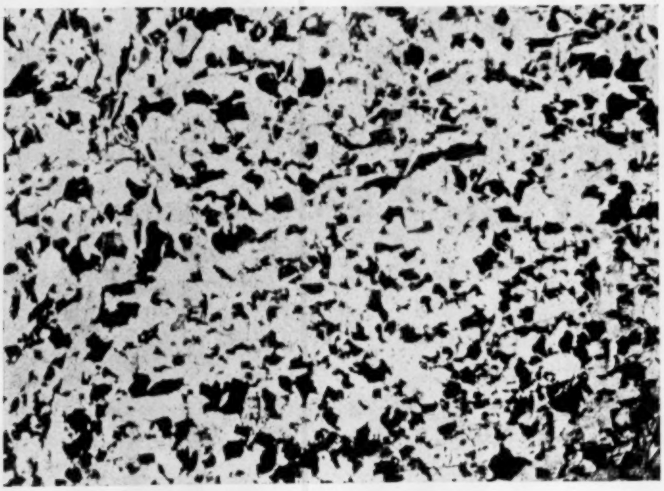

Fig. 2.—Annealed 860° C.

Carbon	0.27 per cent
Manganese	0.54 "
Silicon	0.29 "

Open-hearth Carbon-Steel Casting Coupon (×60).

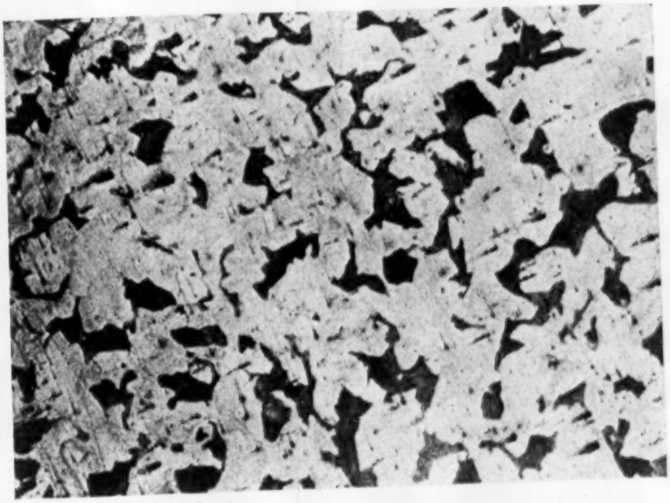

FIG. 3.—As Cast.

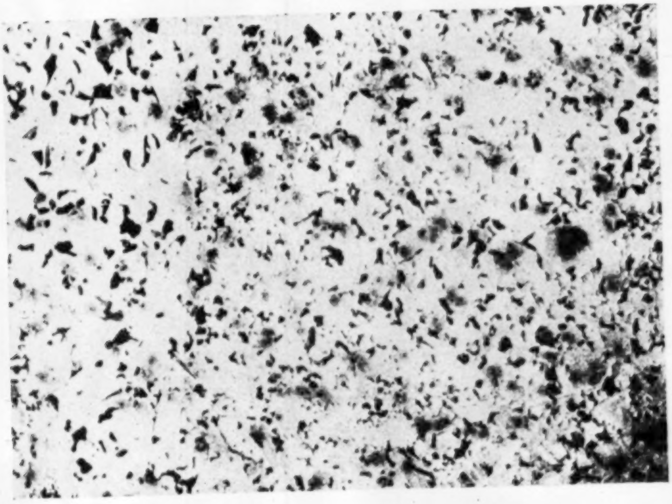

FIG. 4.—Annealed 860° C.

Carbon	0.28 per cent
Manganese	0.58 "
Silicon	0.26 "
Vanadium	0.16 "

OPEN-HEARTH VANADIUM STEEL CASTING COUPON (×60).

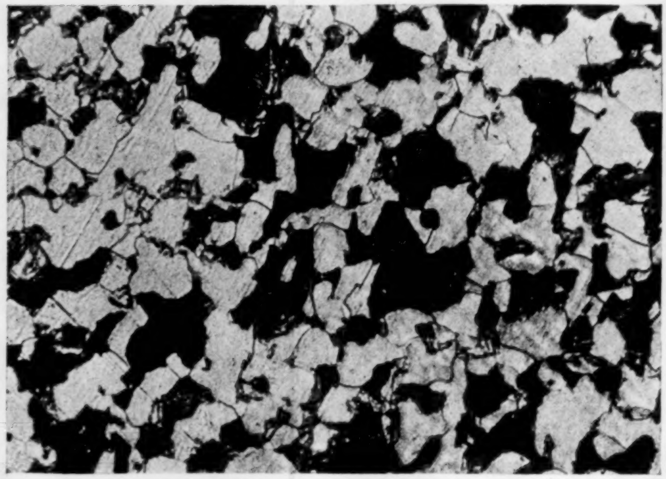

FIG. 5.—Annealed 800° C.

Carbon	0.45 per cent
Manganese	0.48 "

OPEN-HEARTH STEEL BAR (×250).

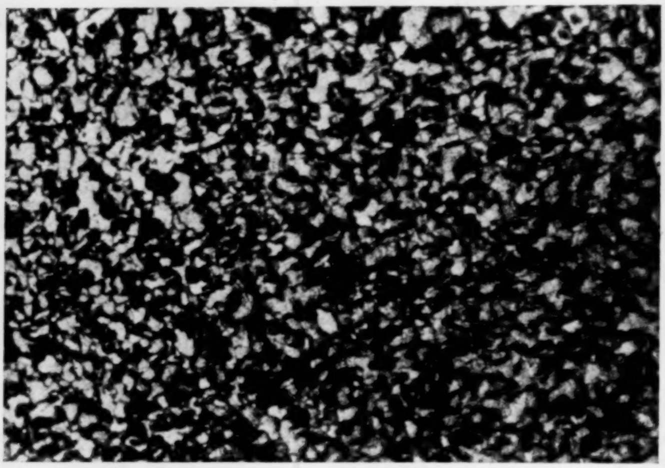

FIG. 6.—Annealed 790° C.

Carbon	0.34 per cent
Manganese	0.87 "
Vanadium	0.13 "

OPEN-HEARTH VANADIUM-STEEL BAR (×270).

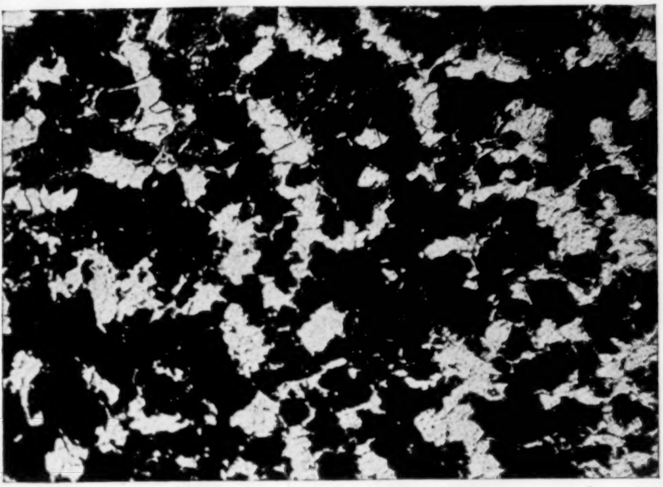

FIG. 7.—Annealed at 800° C.

Carbon	0.43 per cent
Manganese	1.30 "

OPEN-HEARTH STEEL BAR (×250).

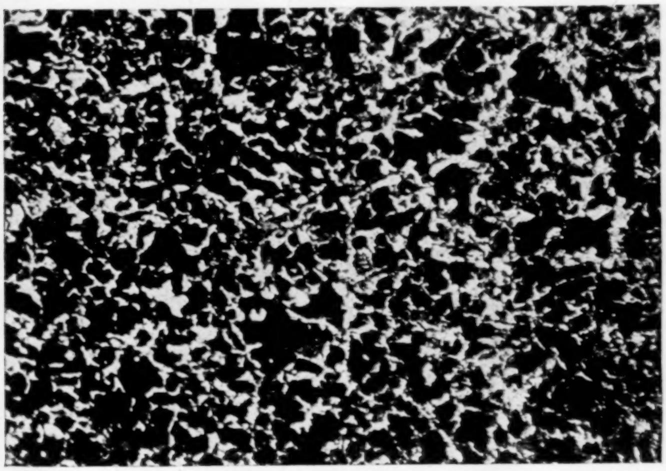

FIG. 8.—Annealed at 800° C.

Carbon	0.47 per cent
Manganese	0.90 "
Vanadium	0.15 "

OPEN-HEARTH VANADIUM-STEEL BAR (×250).

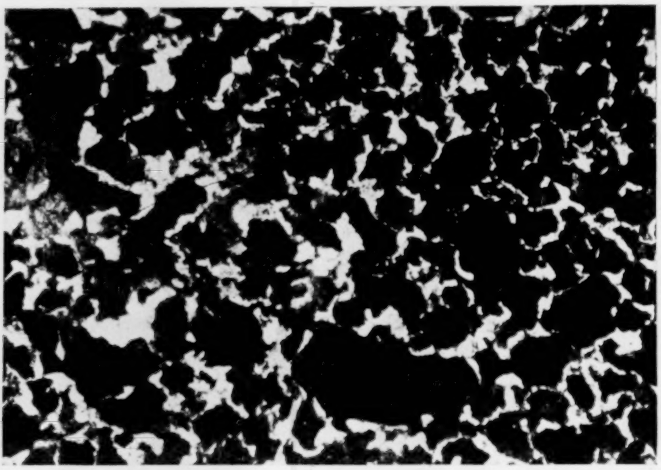

FIG. 9.—Annealed 800° C.

Carbon....................................	0.65 per cent
Vanadium.................................	none

CRUCIBLE STEEL BAR (×600).

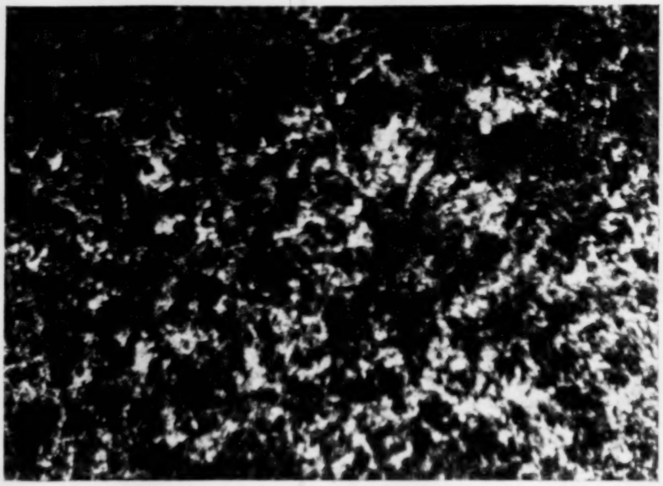

FIG. 10.—Annealed 800° C.

Carbon....................................	0.65 per cent
Vanadium.................................	0.22 "

CRUCIBLE VANADIUM-STEEL BAR (×600).

TABLE I.—PHYSICAL PROPERTIES OF CARBON-VANADIUM FORGING STEEL.

Chemical Composition, per cent.	Kind of Specimen.	Treatment.	Elastic Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.
C. 0.34 Mn. 0.87 V. 0.13	$1\frac{1}{8}$-in. round bar[1]...	As rolled..................	78 000	102 000	23	51
		Annealed 800° C...........	65 000	86 000	28	63
		Oil Q. and T., 900/600° C..	90 000	118 000	25	61
		Oil Q. and T., 875/600° C..	85 000	102 500	22	57
		Water Q. and T., 845/600° C.	93 000	110 000	24	57
		Water Q. and T., 845/565° C.	109 000	125 000	19	60
		Water Q. and T., 845/540° C.	112 500	130 000	19	58
		Water Q. and T., 845/510° C.	115 000	133 350	19	58
		Water Q. and T., 845/455° C.	118 500	145 000	15	51
		Water Q. and T., 845/400° C.	132 500	160 000	12	40
C. 0.37 Mn. 0.96 V. 0.13	1-in. round bar.....	As rolled..................	75 000	102 000	23	57
		Annealed 800° C. in air.....	70 000	90 000	28	63
		Oil Q. and T., 900/650° C..	90 000	114 000	22	64
		Oil Q. and T., 900/600° C..	92 000	118 500	21	63
		Oil Q. and T., 900/540° C..	95 000	120 500	22	62
		Water Q. and T., 830/650° C.	85 000	102 000	26	68
		Water Q. and T., 830/600° C.	97 000	115 000	23	65
		Water Q. and T., 830/540° C.	100 000	120 000	23	67
C. 0.37	$3\frac{1}{4}$-in. hub, outside[2]	Oil Q. and T., 900/595° C..	92 000	119 000	20	55
Mn. 0.96	$3\frac{1}{4}$-in. hub, center[2]	Oil Q. and T., 900/595° C..	87 000	116 000	20	52
V. 0.13	$2\frac{1}{4}$-in. pin, center[2]..	Oil Q. and T., 900/595° C..	95 000	121 000	18	52
C. 0.47 Mn. 0.90 V. 0.15	$1\frac{1}{2}$-in. round bar...	As rolled..................	98 000	135 000	16	39
		Annealed 790° C...........	70 000	108 000	21	51
		Annealed 790° C...........	70 000	107 000	22	53
		Annealed 830° C...........	75 000	107 500	21	48
		Oil Q. and T., 900/700° C..	95 000	120 000	22	61
		Oil Q. and T., 900/650° C..	97 000	125 000	21	60
		Oil Q. and T., 900/600° C..	100 000	128 000	21	58
		Oil Q. and T., 875/430° C..	117 000	137 000	19	52
		Oil Q. and T., 875/315° C..	112 000	139 000	18	51
		Oil Q. and T., 875/200° C..	115 000	142 500	17	45
		Water Q. and T., 830/595° C.	122 000	136 000	20	57
		Water Q. and T., 830/540° C.	130 000	144 000	19	55
		Water Q. and T., 830/430° C.	150 000	172 000	15	44
		Water Q. and T., 830/315° C.	140 000	171 000	11	30
C. 0.47 Mn. 0.90 V. 0.15	7-in. axle..........	As forged..................	68 000	123 000	16	30
		Annealed 790° C...........	52 000	90 000	24	50
		Oil Q. and T., 870/625° C..	82 000	112 500	22	55
C. 0.47 Mn. 0.90 V. 0.15	11-in. axle.........	As forged..................	70 000	117 000	16	28
		Annealed 790° C...........	54 000	94 000	22	47
		Oil Q. and T., 870/625° C..	80 000	115 000	20	52
C. 0.37 Mn. 0.90 V. 0.15	Connecting rod.....	As forged..................	85 000	131 000	17	44
		Annealed 790° C...........	67 000	105 000	23	52
		Oil Q. and T., 870/625° C..	86 000	123 000	20	50
C. 0.47 Mn. 0.90 V. 0.15	11-in. axle, bored with 5-in. hole.....	Annealed 880° C. in air......	63 000	97 000	24	52
		Annealed 880° C. in air......	64 000	99 500	23	51
		Annealed 880° C. in air......	65 000	100 000	23	52

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[1] The Brinell hardness numbers of this specimen were added by Mr. Norris subsequent to the annual meeting. See p. 54.—ED.

[2] Specimen from aeroplane engine crank shaft forged from 4 by 4-in. billet cut from 4 by 16-in. slab.

effect of the original heat treatment and producing inequalities which may result in failure. Probably no other class of forgings is subject to as abusive use as locomotive forgings, and under such conditions ordinary heat-treated forgings have not been found reliable. For these and other reasons annealed forgings are preferred. Consequently, a steel that could give in an annealed condition physical properties equal to or even better than those specified for heat-treated carbon-steel forgings, would prove very desirable.

Carbon-vanadium forging steel responds to heat treatment in a very superior manner, and in this condition will meet the requirements specified by the Society for quenched-and-tempered nickel-steel forgings.[1]

Table I shows the physical properties of carbon-vanadium forging steel under various conditions of section and treatment. Figs. 1 to 10, inclusive, illustrate the marked influence of vanadium on the microstructure of simple carbon steel.

As previously stated, the influence of vanadium on the mechanical properties of quaternary steels is even greater than on simple or carbon steel. The best known and most extensively used of these is chrome-vanadium steel, which forms the subject of Mr. Griffith's contribution to this discussion.

Nickel-vanadium steel, while having possibly even higher tensile properties than chrome-vanadium steel, is considerably more expensive and has not been found to meet all conditions as satisfactorily as chrome-vanadium steel. It does not appear to have as high a resistance to shocks and repeated stresses.

Nickel-chrome-vanadium steel has found extensive application in light armor such as deck plate, gun shields, and armor-piercing projectiles, where its use has greatly improved the efficiency of these materials.

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[1] "Standard Specifications for Carbon-Steel and Alloy-Steel Forgings," (Serial Designation: A 18-16), 1916 Book of A.S.T.M. Standards, p. 141.

THE RÔLE OF CHROME VANADIUM.

By F. J. Griffiths.

Better products require better material, and it was this demand, created by the need of higher-strength steels, that was the foundation for the impetus imparted to investigational activity in metallurgical work in this country.

This research activity has proceeded along definite prescribed channels and the experimental metallurgist, like the alchemist of olden days, engaged in this meritorious occupation, has contributed considerably to the progress of American metallurgy by the discovery of the properties bestowed upon iron when alloyed with chrome vanadium. It is not a little gratifying to contemplate the many problems which have been surmounted in the production of a chrome-vanadium steel of general commercial value, and to witness the growth of this alloy steel through the various stages of its development, until it has assumed such dimensions as to be represented by a large proportion of the total tonnage of accepted standard grades of alloy steels manufactured to-day. The enthusiastic acceptance of this particular alloy steel by the manufacturer of important parts, which are subject to unusual stresses is silent testimony of the merits of this quaternary alloy.

Chrome-vanadium steel holds the position virtually at the head of the commercial alloy steels, and owes this prestige to the extremely wide range of physical qualities which are capable of being developed from this metal by proper methods of heat treatment. Like all alloy steels it is, as its earlier name signifies, a special steel, meaning not only that it is designed for special purposes, but also, that its preparation for the particular work it is called upon to perform must be carried out along certain lines calculated to yield in the final result, those qualities which shall represent 100 per cent efficiency in the final product. Many specifications to-day allow a variation of 1 or 2 per cent in the elongation, and restrict also the ultimate strength and yield point to extremely narrow limits. Examination of these specifications alone would serve to show the wide range of qualities of chrome-vanadium steel, and the absolute delicate accuracy with which by proper management it may be put into condition to perform its highest functions.

Chrome vanadium combines in such a manner with iron, that the separate constituents of the final alloy produced cannot be distinguished, regardless of the methods selected to separate them. Whether we adopt mechanical means or use the microscope under large magnification the result is the same; the discovery of a homogeneous blending of the allied elements which defies detection of the constituent substances. This phenomenon indicates a quite strong combination between the alloying elements and defines the combination unmistakably.

It is generally recognized that when one metal alloys with one or more other metals, its properties are changed in a remarkable manner. The fusion may be lowered, the hardness and strength increased, the ductility affected proportionately and the transformation temperatures raised or lowered, depending upon the nature and characteristics of the alloying elements. Therefore, by properly alloying a metal, the properties of the resulting alloy may possess such profound qualities as to render it more adaptable for the purpose for which it was originally intended. It is the endeavor to decrease injurious properties and to increase the desirable ones. For this fundamental reason, the iron alloys have become to a considerable extent standardized. Those better known to metallurgy and fabricators are the nickel and nickel-chromium steels. Until recently, little investigation has been ventured to ascertain the definite properties of chrome vanadium, and the advantages secured by this alloying element were relatively unknown except to a favorite few, who, through their own research work, acquired intimate knowledge of the characteristics bestowed upon iron by the addition of this alloy.

Chrome vanadium alloys with iron in all proportions and affects the properties to a marked degree, depending upon the percentage of chrome vanadium present. The qualities which become inherent are influenced also more or less proportionately by the amount of carbon contained. In this respect chrome-vanadium steel is a special steel in another sense of the word, and cannot be judged by the same standards as the old carbon steels.

The point of view of the examiner of the old commercial steel is always that of the actual condition in which the metal exists as he sees it before him, and governed by the amount of carbon present. But this point of view has altered gradually until with chrome-vanadium steel, the carbon range permissible to obtain specific characteristics has become narrowed. The presence of chrome vanadium in steel intensifies the effect of the carbon in all the best qualities which it is capable of producing alone. These increases take form not only in physical results, dynamic strength, resistance to wear, increased hardening power, lack of brittleness, etc., but also in purity, soundness and homogeneity of the metal. These are the specific contributions which chrome vanadium brings to iron as compared to the older simple steel.

Unfortunately, many of these qualities enumerated have no current and accepted method of measurement, whereby they may be offered on a basis for comparison. Figures may be, and often are, offered to show accurately the result of comparative tests which are intended as a means of comparison of the functions of a particular steel, but, ordinarily, the test conditions would vary so much under the manipulation of other experimenters that no comparable results are really obtainable. In any event, the data submitted usually mean nothing to the other man.

The property of soundness of the casting and ingots, produced by the use of chrome vanadium, induces freedom from occluded gases and greatly increases the desirable physical qualities of the forgings subsequently made from this steel, such as strength, ductility and resilience. These desirable properties are amply proved not only from the results obtained from tests, but also from the more vital trial of actual use. The user of an alloy steel is concerned entirely with what ultimate qualities the metal is capable of developing, regardless of the experience with which he may have previously contended.

According to the old view and judging from the results of tests chrome-vanadium steel, as rolled or annealed, may appear to be a worthless product. It may have, as in examples to be given later, a percentage of elongation, reduction of area, elastic limit and ultimate strength corresponding to simple carbon steel. Stress is laid on this particularly, because there is still a large and unreasonable prejudice in certain quarters against the use of steels which have to be put in special condition, or more plainly, heat treated, before they are capable of doing their best work. Of course, as we all know, this feeling is largely giving way before the splendid performance which steels of this character are offering in service, and given sufficient time, the special steels themselves will demonstrate the unsoundness of this objection, based largely upon the limitations of the older simple carbon steels.

In order to place the foregoing more definitely before you, in figures rather than in words, your attention is invited to the sub-joined tests, which are in no sense exceptional. They have been selected almost at random from records merely for the purpose of illustration. You will note that they cover briefly four classes of chrome-vanadium steel, as follows:

1. A steel with a composition of about 0.31 per cent of carbon, 0.95 per cent of chromium and 0.14 per cent of vanadium;
2. A steel of 0.42 to 0.49 per cent of carbon, approximately 1.10 per cent of chromium, and 0.15 per cent of vanadium;
3. High-carbon chrome-vanadium steel of a composition of 0.98 per cent of carbon, 1.06 per cent of chromium and 0.12 per cent of vanadium; and
4. Low-carbon chrome-vanadium steel of an analysis suitable for most structural purposes, namely, carbon, 0.24 per cent; manganese, 0.69 per cent; chrome, 1.10 per cent, and vanadium, 0.15 per cent.

It is to be regretted that while we are engaged on extensive tests of the third class of material mentioned, our experimental data are not at present available for use here. However, the extreme softness and ductility of this steel in the annealed condition may be noted from the one example given. This result is rather surprising in view of the high-carbon content present. Table I gives results of tests on these steels.

After an examination of these results, which are concerned only with the simplest quenching and drawing treatments, little comment is needed to draw attention to what might be termed the versatility of chrome-vanadium steel. One comment, however, based upon a practical consideration and not upon theory, may well be made here. Test results such as these just presented

TABLE I.—PHYSICAL PROPERTIES OF STEELS OF VARIOUS CHEMICAL COMPOSITIONS.

Chemical Composition.				Physical Properties.						Remarks.
Carbon, per cent.	Manganese, per cent.	Chromium, per cent.	Vanadium, per cent.	Yield Point, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Brinell Hardness Number.	Drawing Temperature, deg. Cent.	
0.31-PER-CENT CARBON CHROME-VANADIUM STEEL.										
0.31	0.71	0.95	0.145	238 000	255 000	6.5	26.0	444	200	Heated to 850° C. Cooled to 800° C., quenched in water. Reheated to the temperatures stated for 20 min., and cooled in air.
				220 000	232 000	10.5	49.0	444	260	
				178 000	193 000	11.0	50.0	444	375	
				170 000	179 000	12.5	55.0	387	430	
				153 000	166 000	15.0	58.0	364	485	
				149 000	160 000	16.0	59.0	351	540	
				130 000	146 000	17.0	60.5	340	595	
0.31	0.67	0.96	0.137	225 000	234 000	6.0	14.0	321	200	Heated to 850° C. Cooled to 800° C., quenched in oil. Reheated to the temperatures stated for 20 min., and cooled in air.
				212 000	223 000	7.5	16.0	321	260	
				206 000	212 000	8.5	41.0	321	320	
				167 000	175 000	11.5	52.0	321	430	
				148 000	159 000	13.0	55.0	303	485	
				141 000	151 000	15.0	60.0	286	545	
				130 000	144 000	16.5	62.0	269	595	
0.42 TO 0.49-PER-CENT CARBON CHROME-VANADIUM STEEL.										
0.48	0.86	1.16	0.14	100 000	120 000	22.0	62.0	260	700	Heated to 875° C. Cooled to 825° C., quenched in oil. Reheated to the temperatures stated for 20 min., and cooled in air.
				116 000	131 000	19.0	58.0	286	656	
				155 000	170 000	15.0	45.0	387	650	
				173 000	185 000	12.0	40.0	418	580	
0.49	0.84	1.07	0.132	167 000	179 000	12.0	35.0	418	540	Heated to 900° C. Cooled to 825° C., quenched in oil. Reheated to the temperatures stated for 20 min., and cooled in air.
				182 000	188 000	11.0	32.0	444	485	
				202 000	216 000	10.0	31.0	477	430	
				220 000	228 000	9.5	30.0	555	375	
0.45	0.69	0.82	0.258	122 000	142 000	16.0	55.0	321	...	As rolled.
0.42	0.86	1.15	0.145	97 000	136 000	19.0	60.0	321	...	As rolled.
HIGH-CARBON HIGH-CHROME STEEL.										
0.98	0.42	1.06	0.12	43 200	92 450	32.0	64.0	170	...	Annealed.
LOW-CARBON CHROME-VANADIUM STEEL.										
0.241	0.69	1.10	0.15	194 000	209 000	12.0	51.0	444	200	Heated to 850° C. Cooled very slowly to 800° C., quenched in water. Reheated to the temperatures stated for 20 min., and cooled in air
				197 000	206 000	11.1	53.0	444	260	
				182 000	198 000	10.0	53.0	444	320	
				166 000	179 000	13.0	57.0	402	375	
				161 000	169 000	15.0	58.0	387	430	
				148 000	157 000	16.0	59.0	364	485	
				144 000	155 000	18.0	60.0	340	540	
				130 000	142 000	20.0	63.0	332	595	
0.224	0.61	0.85	0.153	158 000	162 000	14.0	57.0	364	430	Heated to 850° C., Cooled very slowly to 800° C., quenched in water, Reheated to the temperatures stated for 20 min., and cooled in air.
				142 000	150 000	15.0	61.0	340	485	
				135 000	146 000	16.0	62.0	321	540	
				128 000	139 000	19.0	64.0	302	605	

are valuable only in so far as they are recognized for what they are, namely, test results. They represent, it is true, what it is possible to do with the metal when it is in the finished state, but they do not necessarily represent what the qualities of the metal in the final product will be, if the heat treatment is carried on under exactly the same conditions as those which obtain in the tests. When it comes to question of heat treatment of the final product, the conditions must of necessity be altered somewhat, and the treatment of each class of articles then becomes a special problem. This should not be considered in the light of admission on the part of the steel manufacturer; it should be rather conceded as the principle by which he is most willing to work.

One of the principal effects produced in steel by the addition of chrome vanadium, is to raise the temperature at which the carbides dissolve in the iron, and the temperature at which they are precipitated out of solution. This elevation of the solution temperature is quite noticeable, the carbon content affecting this temperature but slightly. In connection with the question of the position of the critical ranges of chrome-vanadium steel, Figs. 1, 2, 3 and 4 show both the heating and cooling curves obtained by means of a Leeds & Northrup transformation-point apparatus. Upon reference thereto, a quite remarkable characteristic is demonstrated. The peak of each curve on heating occurs between 770 and 793° C. and the greatest extension of the cooling curve is found to branch out at about 695° C. These curves, taken with the Leeds & Northrup apparatus mentioned above, indicate the temperature relationship between the sample being tested and a uniformly heating body heated together in the same furnace. In the chart the actual temperatures are plotted as abscissas and the temperature differences as ordinates.[1] The temperature differences, however, are not given definite values in degrees in these curves inasmuch as this is taken care of by the instrument itself, which is specifically designed to record this difference autographically. These four steels have respective carbon contents of 0.18, 0.22, 0.49, and

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[1] In the chart plotted by the instrument the actual temperatures are ordinates and the temperature differences abscissas, but for convenience of publication this arrangement has been reversed, as noted above.—ED.

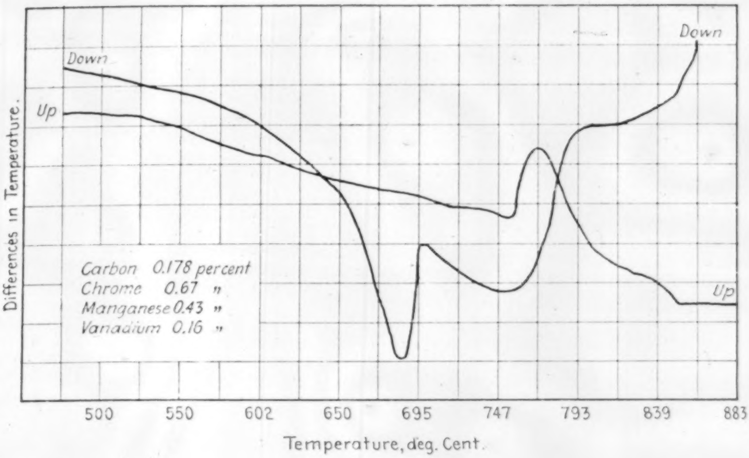
Down
Up
Down
Up
Differences in Temperature.
Carbon 0.178 percent
Chrome 0.67 "
Manganese 0.43 "
Vanadium 0.16 "
500
550
602
650
695
747
793
839
883
Temperature, deg. Cent.

FIG. 1.

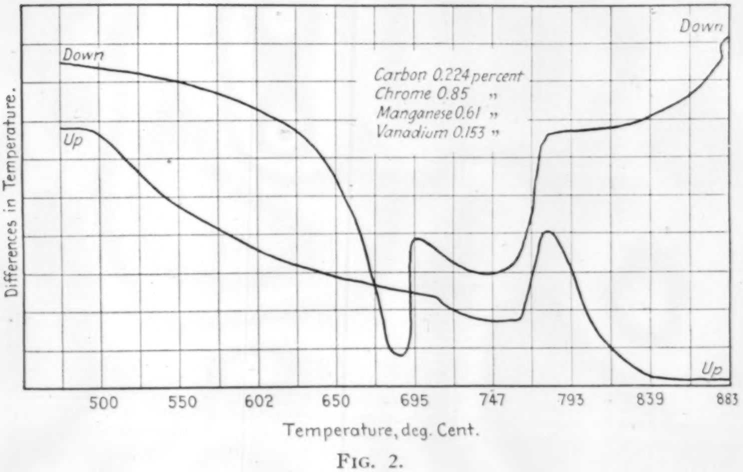
Down
Up
Down
Up
Differences in Temperature.
Carbon 0.224 percent
Chrome 0.85 "
Manganese 0.61 "
Vanadium 0.153 "
500
550
602
650
695
747
793
839
883
Temperature, deg. Cent.

FIG. 2.

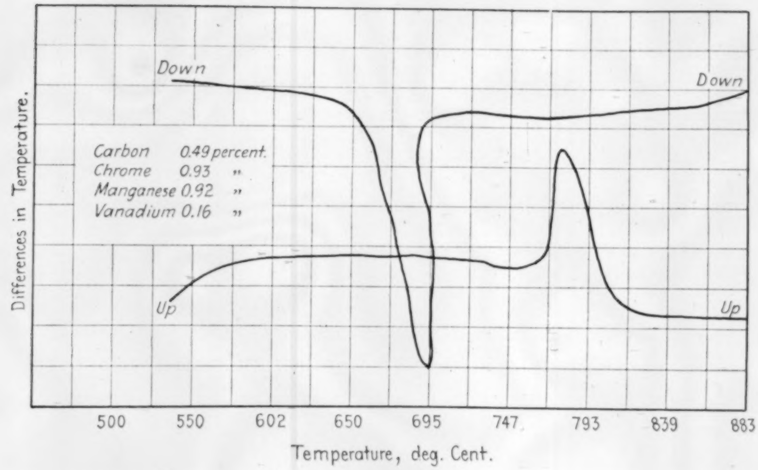
Differences in Temperature.
Down
Down
Up
Up
Carbon 0.49 percent.
Chrome 0.93 "
Manganese 0.92 "
Vanadium 0.16 "
500
550
602
650
695
747
793
839
883
Temperature, deg. Cent.

FIG. 3.

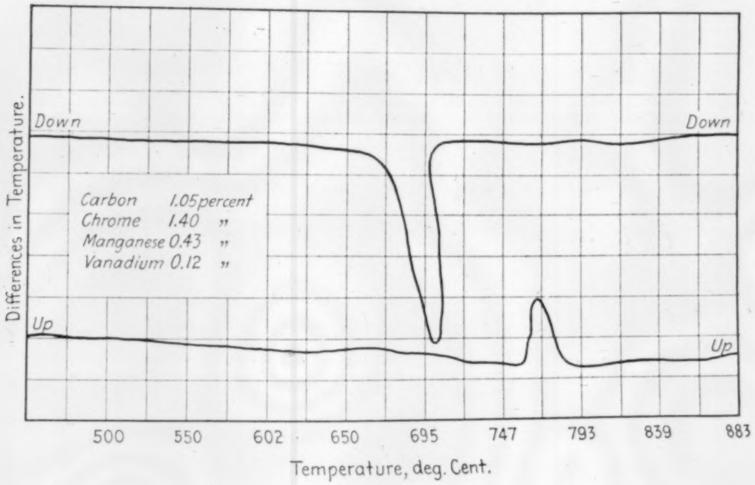
Differences in Temperature.
Down
Down
Up
Up
Carbon 1.05 percent
Chrome 1.40 "
Manganese 0.43 "
Vanadium 0.12 "
500
550
602
650
695
747
793
839
883
Temperature, deg. Cent.

FIG. 4.

1.05 per cent, with varying chromium and vanadium contents. They show the wide transformation ranges at the high temperatures characteristic of this alloy. It will be noted that the range of temperature intervening between the completion of the upper transformation on heating and the completion of the lower transformation on cooling is approximately as follows:

Chrome-vanadium steel with 0.18 per cent of carbon.... 183° C.
" " " " 0.22 " " " " .... 170° C.
" " " " 0.49 " " " " .... 138° C.
" " " " 1.05 " " " " .... 100° C.

Figs. 1 to 4, inclusive, furnish conclusive evidence that when chrome vanadium is added to steel, it raises the critical points on heating above the corresponding carbon-steel temperatures, but on cooling the critical point is considerably depressed, roughly in proportion to the maximum temperature from which the steel is cooled. The higher the maximum temperature reached the lower the critical range will be on cooling. This fact serves to show the slowness with which the structural changes occur in this alloy and the additional time required for the transformation to be effected completely.

It is unnecessary to point out the practical utility in commercial practice of working with a steel, in which this slow rate of transformation can be taken advantage of by the use of higher temperatures without resulting injury to the product, and also by the fact that the steel can stand exposure at these higher temperatures without the attendant harmful effects characteristic of other steels subjected to the same conditions. In fact, chrome vanadium seems to attain its greatest combination of desirable qualities at higher treatment temperatures than would be expected from the location of its critical range, due principally to the greater length of time necessary to effect the complete transformation of its constituents into a uniform and homogeneous solid solution. This point is emphasized because certain other commercial alloy steels, although excellent in many respects, have very narrow limits of annealing and hardening temperatures, and therefore require unusual care in their treatment, a slight deviation from the correct temperatures resulting unsatisfactorily.

A single quenching and drawing treatment, as we have seen from the tests submitted, is sufficient for general purposes to effect complete refinement and hardening of chrome-vanadium steel. This characteristic of simplicity in treatment obviously insures a high-grade product under ordinary commercial conditions, and when compared with the double and triple heat treatments required for the refinement of other alloy steels, the economy presented is obvious. These properties, possessed only by the chrome-vanadium alloy, characterize this steel as a material universally applicable for any commercial requirement. They also simplify the treating instructions necessary to successfully handle alloy steel of this chemical composition. One heat-treating specification is adaptable for any carbon analysis, whether the steel is destined for case-hardening purposes or for oil-treating for gears and cones, etc. It has been found by experience that a temperature of 775° C., which is suitable for hardening a case-hardening steel and still retain desirable toughness of the core, can also be employed for hardening oil-treated parts.

The lower-carbon chrome-vanadium steels are apparently pearlitic in nature and, as such, possess the increased strength and hardness resulting from the effect of the special elements, in producing a ferrite of superior strength, subsequently resulting in a more dense and improved pearlite. With the increase in the carbon and alloy contents, this steel becomes more or less cementitic and it is probably this quality which demands the use of higher temperatures than usual in treatment in order to produce complete absorption of its component parts.

The quality of chrome-vanadium steel often described as "tough hardness" is one of its especially desirable properties, and is due probably to the somewhat finer and denser structure of the microscopic constituents, and to their more uniform distribution. This effect is obtained not only through the actual influence of the chromium and vanadium as component parts of the alloy, but also to the influence of the combination of both of these elements in producing in the molten metal a condition of purity and freedom from gases, and, therefore, subsequently forming a metal which, at the time when the structural changes occur, is in the best possible condition for these changes to take place, that is, in the highest form in which the alloy composition renders it possible.

We have found then from the foregoing remarks, substantiated by conclusive tests, that chrome-vanadium steel can be truly described as a universal steel. With the carbon present in variable amounts, almost any physical property desired can be secured. It is adaptable for case hardening where the carbon should not exceed 0.20 per cent. For axle shafts, etc., where nominal strength is essential; for oil-treated gears and vanadium springs, where great resilience and resistance to repeated shock is required; for dies, where surface hardness and toughness must both be present; for drills, etc., where wear-resisting qualities and lack of potential brittleness is necessary; for cones and raceways of bearings, which must possess a glass-hard surface, accompanied with proportionate toughness to prevent checking and cracking under the stresses imposed, chrome-vanadium steel is employed to advantage.

TABLE II.—RESULTS OF TESTS ON CHROME-VANADIUM STEELS.

Specimen No.	Yield Point, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Brinell Hardness No.
1....................	162 820	172 850	14	49.7	418
2....................	160 130	172 090	14	49.5	418
3....................	160 840	172 090	15	49.9	387
4....................	159 520	171 630	14.5	46.9	375
5....................	154 480	164 760	14	52.4	364
6....................	151 000	162 240	14	55.7	364
7....................	154 570	165 160	15	52.3	364
8....................	153 920	163 590	15	54.0	351
9....................	146 590	162 790	15	55.6	340
10....................	146 700	156 360	15	55.7	340
11....................	146 080	157 790	15.5	55.3	321
12....................	148 620	158 090	15.5	55.8	332

The adaptability of chrome-vanadium steel is general, and where strength and hardness and resistence to fatigue properties are the prerequisite, there is a chrome-vanadium steel for the purpose. When the steel is case hardened, a glass-hard surface and tough fibrous core is easily obtained. These properties are obtained without complex treatment, consisting of simple quenching, with reheating and quenching. For other heat-treated parts, a simple heating, quenching and drawing gives a steel, relatively so tough and strong, that it is unlike any other hardening alloy steel. Chrome-vanadium steel is a universal alloy steel and stands forth unmistakably as the master alloy.

Table II gives results of tests made by a large automobile manufacturer using chrome-vanadium steel for principal parts. The tests given represent those made from 0.30 to 0.40-per-cent carbon chrome-vanadium steel used for axle shafts, and demonstrate results that are secured from routine practice. The uniform strength exhibited supplies incontestible testimony of the dependable qualities inherent in this analysis steel when treated in commercial quantities.

DISCUSSION.

Discussion on Nickel.

Mr. Henry M. Howe (*by letter*).—The quantity of carbide in solution in any hardened carbon-iron alloy is so extremely small that I doubt whether we can reasonably assign to it any appreciable weight in influencing the tensile properties. Slight irregularities in the curves such as Mr. Abbott's, shown in his Fig. 1, are not, I think, more than should be expected from the intermingling of two components such as ferrite and cementite, having such very different properties. Should we, for instance, be surprised to learn that a variation in the proportion of sand and cement in concrete produced a straight-line variation in the properties of the concrete? **Mr. Howe.**

Discussion on Silicon.

Mr. C. W. Burrows.—In Mr. Ruder's paper, the following statement referring to percentages of silicon between 2 per cent and a trace appears: "The silicon merely acts as a diluent causing a steady drop in saturation value and permeability and an increase in coercive force." The first two statements are obvious, but I cannot see why an increase in coercive force should be an indication that the silicon is a diluent. **Mr. Burrows.**

Mr. W. E. Ruder.—In answer to Mr. Burrows' question, I would say that in my opinion anything added to iron as an alloying element which does not increase the grain size will increase the coercive force; and any material or compound, such as slag, carbon, or sonims, that is in solution, or mixed with the steel, will increase the coercive force. Another reason for that statement is the general fact that, although there are exceptions, anything which decreases the permeability increases the hysteresis, and, consequently, the coercive force. **Mr. Ruder.**

Mr. R. R. Abbott (*by letter*).—Mr. Ruder says that the strength of pure iron is increased from 38,000 to about 90,000 lb. per sq. in. by 4 per cent of silicon. It may be interesting to note **Mr. Abbott.**

Mr. Abbott. that my formulas for the effect of silicon on pure iron give a calculated value of 93,480 lb. per sq. in. for a 4-per-cent silicon alloy, which agrees closely with Mr. Ruder's figures. My formulas give an elastic limit for a 4-per-cent silicon steel of 62,500 lb. per sq. in. However, I am not entirely in accord with Mr. Ruder's figures for reduction of area and elongation, as my formulas give for 2.5 per cent of silicon a drop in percentage of reduction from 72.5 to 61.0 and a drop in percentage of elongation from 46.5 to 36.5.

Mr. Ruder states that carbon steels containing 0.2 to 0.4 per cent of silicon are used for automobile springs. The ordinary type of silicon steel used for automobile springs contains from 1.8 to 2.2 per cent of silicon.

Mr. Ruder. MR. RUDER (*Author's closure by letter*).—Referring to Mr. Abbott's discussion, I wish to say that the figures which I gave for the mechanical properties of iron-silicon alloys were intended, not as specific values, but only to indicate the general effect of silicon. Mr. Yensen's values, referred to in the paper, show that for pure annealed alloys the ultimate strength is 91,600 lb. per sq. in. at 4.44 per cent silicon, or about 86,000 lb. at 4 per cent. Other investigators have given values of from 76,000 to 87,000 lb. None have exceeded this last value. It would appear, therefore, that Mr. Abbott's calculated value of 93,480 lb. per sq. in. is rather high.

Actual figures for reduction of area show a decrease from 81 to 75 per cent for 2.5 per cent of silicon. Again referring to Mr. Yensen's results, he finds that there is an immediate rise in value to about 90 per cent, this value persisting until about 2.5 per cent of silicon is added. Other investigators testing less pure alloys, give curves which fall more or less regularly from 72–70 to 25 per cent for 2.5 per cent of silicon. These latter values will be found to correspond more closely with Mr. Abbott's calculations. In percentage elongation the values are 55 to 45 per cent at 2.5 per cent of silicon. Mr. Yensen here gives a drop from 58 to 45 per cent for 2.38 per cent of silicon. Paglianti, Baker and Hadfield give values varying from 45–47 to 18–38 per cent. These figures again agree with those calculated by Mr. Abbott more closely than those obtained on purer materials.

With regard to the amounts of silicon used for springs, it is true that specifications for silico-manganese steels, for example, call for silicon as high as 2 per cent. **Mr. Ruder.**

DISCUSSION ON VANADIUM.

Mr. Abbott. MR. ABBOTT.—Mr. Norris states that a vanadium steel containing cementite does not readily occur as thin plates in the pearlite but in a granular or sorbitic condition. My experience has been that whenever we wish to obtain a particularly good example of lamellar pearlite we carbonize a chrome-vanadium steel and cool it slowly. Whether the chrome or the vanadium causes the more perfect formation of lamellar pearlite is not important, however; the presence of the vanadium certainly in this case at least does not prevent the formation of this perfect lamellar pearlite.

Mr. Norris states: "Yet they (vanadium-carbon steels) have mechanical properties equal to those of ordinary 3-per-cent nickel steel." This is evidently a mistake unless I misunderstand the statement. Certainly a steel containing in the neighborhood of 0.2 per cent of vanadium is not the equal in physical properties of a 3-per-cent nickel steel of the same composition otherwise.

The physical properties given by Mr. Norris in Table I of his paper are practically what we get in a steel corresponding to the chemical composition given, even if the vanadium is not present. The higher physical properties shown in this table over the ordinary carbon steel would appear to be due almost entirely to the use of a high percentage of manganese. The ordinary carbon forging stock has a manganese content of about 0.50 per cent. The steels given in Mr. Norris' Table I have a manganese content of about 0.90 per cent and a vanadium content of about 0.14 per cent. These steels therefore contain nearly twice as much manganese as the ordinary forging steel. Now the relative effects of vanadium and manganese upon the physical properties of steels vary with the heat treatment. Consider a single example: In annealed steels the tensile strength is increased approximately 210 lb. per sq. in. by 0.01 per cent of vanadium and 150 lb. per sq. in. by 0.01 per cent of manganese; therefore the annealed steels in this table owe 13,500 lb. per sq. in. of their tensile strength to the manganese present and

Mr. Abbott. 2940 lb. per sq. in. to the vanadium; or considering the effect of only that amount of manganese in excess of the amount normally present in ordinary forging steel this excess manganese has added 6000 lb. per sq. in. to the tensile strength. In Fig. 1 I have given the physical results obtained from two steels of practically the same chemical contents, except that one has an

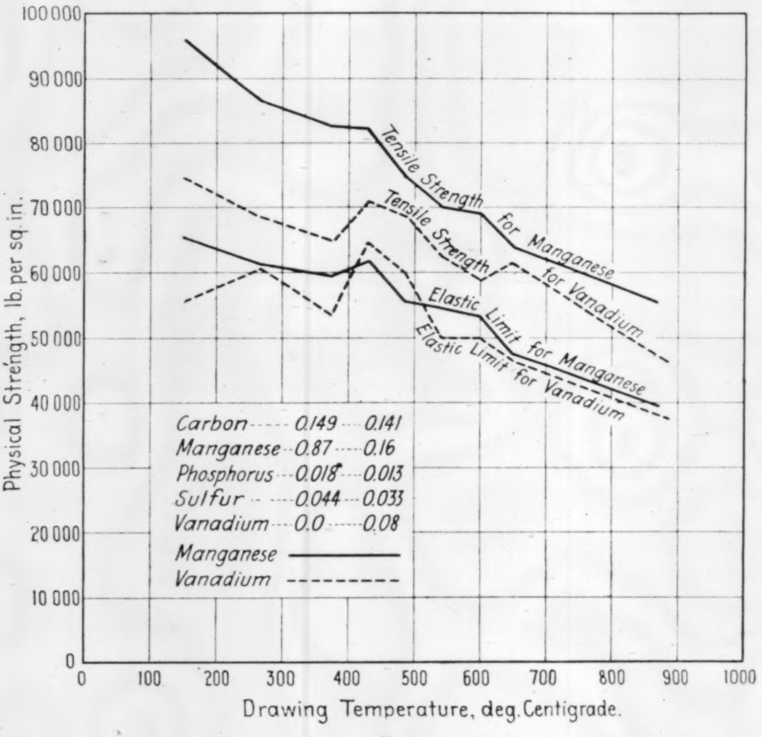

FIG. 1.

excess of manganese and no vanadium while the other contains vanadium.

Vanadium, weight for weight, is more powerful in increasing the physical properties of annealed forging steels than any other of the common elements found in ordinary steels excepting carbon (and phosphorous in small amounts); however, the small amounts which are normally used exert no remarkable effect upon the physical properties. This statement is also true in general for heat-treated steels. As an example: considering the effect of the same amounts of the various elements upon the tensile strength of annealed steels with carbon contents less than 0.45 per cent and calling the effect of vanadium unity, we have the following table: **Mr. Abbott.**

Carbon	4.19
Vanadium	1.00
Silicon	0.76
Manganese	0.70
Nickel	0.20
Chrome	0.17

This table represents the variations only when the normal amounts of these elements are present.

Mr. Acker. MR. E. O'C. ACKER.—I wish to take issue with Mr. Norris on the last paragraph of his paper, in which he says: "Nickel-chrome-vanadium steel has found extensive application in light armor, such as deck plate, gun shields, and armor-piercing projectiles, where its use has greatly improved the efficiency of these materials." The best armor-piercing projectiles being furnished to-day contain no vanadium; they are made of chrome-nickel steel. The two manufacturers who stand at the head of the list in the quality of their projectiles do not use vanadium at present, nor have they ever used it. A similar condition exists in regard to the light armor of which Mr. Norris speaks, which is called special treatment steel in the Navy specifications. This steel to-day is being made without vanadium by several manufacturers, and in the last three weeks I witnessed the ballistic testing of a plate 4 in. thick, which was equal to any chrome-nickel-vanadium plate of the same dimensions I have ever seen tested, and superior to many of them.

It is extremely unlikely from the evidence that the addition of vanadium to chrome-nickel steel has "improved the efficiency of these materials."

Mr. Norris. MR. G. L. NORRIS.—Replying to Mr. Abbott's criticisms I would say that his description of how he produces specimens of lamellar pearlite from chrome-vanadium steel confirms rather than disproves the statement that vanadium containing cementite does not readily occur as thin plates in the pearlite, but in a

Mr. Norris. granular or sorbitic condition. His specimens were not "readily" produced, but were the result of an unusual, slow and drastic treatment. By "readily" I mean under the ordinary and usual conditions that steel is handled. This characteristic of vanadium cementite was first mentioned by J. O. Arnold, and has been noted by a number of subsequent investigators since. It is characteristic of pure iron carbide or cementite to form thin plates, and beautiful specimens of lamellar pearlite can generally be obtained from 0.40 to 0.60-per-cent carbon-steel bars as rolled.

I have found that in general the statement that simple carbon-vanadium forging steels, such as I have described, are equal to "ordinary" 3-per-cent nickel steels is correct, with the possible exception of when drawn back at very low temperatures. The tests given in Table I are in confirmation of this.

TABLE I.

Test No.	Carbon, per cent.	Manganese, per cent.	Nickel, per cent.	Vanadium, per cent.	Treatment. (Temperatures, deg. Cent.)	Elastic Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent	Reduction of Area, per cent.	Treated.
1...	0.34	0.71	3.35	....	WQ & T 770/593	103 000	109 000	25	69	$\frac{3}{4}$-in. Rd.
			3.35	....	" 770/538	105 000	121 000	23	65	" "
2...	0.34	0.87	....	0.13	" 845/600	93 000	110 000	24	57	$1\frac{1}{8}$-in. Rd.
3...	0.34	0.87	....	0.13	" 845/540	112 500	130 000	19	58	" "
4...	0.33	0.55	3.17	....	" .../540	113 000	120 000	22	63	$\frac{3}{4}$-in. Rd.
5...	0.30	0.40	3.36	....	OQ & T 900/540	66 210	97 780	25	59	1-in. Rd.
6...	0.26	0.42	....	0.23	" 900/540	70 280	96 770	25	59	" "

References:
Test No. 1.—R. R. Abbott.
" " 2.—Flannery Blot Co.
" " 3.—M. A. Ammon, Willys-Overland Co.
" " 4.—R. R. Abbott, *Proceedings*, Am. Soc. Mech. Engrs., 1915.
" Nos. 5 and 6.—Bethlehem Steel Co.

I believe that the elastic limit given in the first test of test No. 1 must be wrong, as the elastic ratio is considerably greater than in the second test of the same steel with a lower draw back or temper. Assuming the same elastic ratio for the first test as was obtained in the second test, 0.867, which is about what it should be, the elastic limit would be 94,500 lb. per sq. in., which I believe is more nearly correct.

The tests of the vanadium steel are not as high as if made

Mr. Norris.

on as small a section as in the case of the nickel steels and hence do not show to as great an advantage as they otherwise would.

Recent tests of the vanadium steel just compared with the nickel steel in the form of helmets shows it to be superior to $3\frac{1}{2}$-per-cent nickel steel under the ballistic tests.

Relative to Mr. Abbott's remark that the physical properties which I give for the carbon-vanadium forging steels are due almost entirely to the manganese, and that the small amounts of vanadium normally used exert no remarkable effect, I would submit the tests given in Table II. I would also mention that several forging concerns are using simple carbon forging steel of ordinary analysis to which about 0.08 per cent of vanadium has been added and are very well satisfied that the steel has been materially improved thereby.

TABLE II.

Test No.	Carbon, per cent.	Manganese, per cent.	Vanadium, per cent.	Treatment. (Temperatures, deg. Cent.)	Elastic Limit, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Treated.
1...	0.275	0.55	....	OQ & T 900/540	49 910	80 470	32	65	1-in. Rd.
2...	0.26	0.42	0.23	" 900/540	70 280	96 770	25	59	" "
3...	0.34	1.61	....	WQ & T .../540	116 000	125 000	18	51	$\frac{3}{4}$-in. Rd.
4...	0.34	0.87	0.13	" 845/540	112 500	130 000	19	58	$1\frac{1}{8}$-in. Rd.
5...	0.37	1.52	0.20	" 900/600	144 900	153 000	17	51	1-in. Rd.

References:

Test Nos. 1 and 2.—Bethlehem Steel Co.
" No. 3.—R. R. Abbott, *Proceedings*, Am. Soc. Mech. Engrs., 1915.
" " 4.—M. A. Ammon, Willys-Overland Co.
" " 5.—Carnegie Steel Co.

The test from the $1\frac{1}{8}$-in. round bar does not show as high physical properties as if treated in a $\frac{3}{4}$-in. section like the steel with which it is compared. Here we find that the vanadium steel with 0.87 per cent manganese is equal to a simple steel with 1.61 per cent manganese, or that 0.13 per cent vanadium is equivalent to 0.74 per cent manganese, and not 0.20 per cent according to Mr. Abbott. Or instead of being $1\frac{1}{2}$ times more powerful, vanadium is apparently 5.7 times more powerful than manganese.

I maintain from my own experience and that of others that the small amounts of vanadium normally present in steels do have a very great effect on the physical properties. This has been

Mr. Norris. so thoroughly established and generally accepted during the past ten or more years that it seems strange to have Mr. Abbott take this stand, while at the same time admitting that "vanadium, weight for weight, is more powerful in increasing the physical properties of annealed forging steels than any other of the common elements found in ordinary steels excepting carbon."

Mr. Acker's remarks about the use of vanadium in armor-piercing projectiles and thin armor plate amount to a general denial that it is used for these purposes. Owing to Mr. Acker's connection with one of the largest producers of such material in this country, I am naturally placed in a somewhat embarrassing position. However, I propose to show that Mr. Acker is mistaken.

In Table III are given analyses of recently manufactured 12 and 14-in. armor-piercing projectiles by three concerns in this country, including the company with which Mr. Acker is connected, which show that vanadium is used for this purpose.

TABLE III.

Size of Shell, in.	Carbon, per cent.	Manganese, per cent.	Silicon, per cent.	Chromium, per cent.	Nickel, per cent.	Vanadium, per cent.
12	0.82	0.29	0.24	2.73	2.25	0.11
12	0.81	0.30	0.20	1.29	3.94	0.10
14	0.72	0.44	0.24	2.66	3.19	0.08
14	0.64	0.64	0.20	2.30	3.91	0.07
14	0.75	0.49	0.22	1.46	4.22	0.07
14	0.52	0.30	0.29	2.54	2.72	0.07
14	0.80	0.33	0.24	2.86	3.68	0.08
14	0.71	0.25	0.22	1.86	3.79	0.06

Most of the bullet-proof steel, gun-shield steel, made in this country and England, and I believe prior to the war in Germany also, is to the following typical analysis:

Carbon	0.37	per cent.
Manganese	0.35	"
Nickel	3.30	"
Chromium	1.15	"
Molybdenum	0.25	"
Vanadium	0.25	"

Mr. Norris. Recent analyses of gun-shield steel from four makers in this country shows vanadium present in all cases and ranging from 0.18 to 0.24 per cent. Thousands of helmets for the English army have been made from nickel-chrome-molybdenum-vanadium steel of the gun-shield composition, and the following analysis of a German helmet shows that they have a similar steel:

Carbon	0.396 per cent.
Manganese	0.34 "
Nickel	4.11 "
Chromium	1.22 "
Vanadium	0.24 "

According to a recent issue of *The Iron Age*, there was exported during the eleven months ending May, 1917, 2,421,072 lb. of ferro-vanadium. I am positive from my knowledge of the destination of a lot of this ferro-vanadium that much of it was used in the manufacture of special-treatment steel or deck plate.

It is evident that foreign governments like England and Italy using these plates consider that vanadium improves them, for it was only after extensive tests that they went from nickel-chrome to nickel-chrome-vanadium steel. It is reasonable to suppose that under the present financial stress they would not stand the additional expense of vanadium if the plates were not better through its use.

I believe that the analyses and other information above confirms the paragraph to which Mr. Acker takes exception.

Mr. Howe. MR. HOWE (*by letter*).—If Mr. Norris could give us the Brinell hardness data for the specimens represented in his Table I, that would increase the value of the table greatly.

The value of the paper could be increased further by showing graphically the influence of the drawing temperature on the several series of specimens quenched and tempered, Table I.

There is a striking contrast between Fig. 1 and 3 in that the latter lacks the prominent Wiedmannstattian straight beams of ferrite common in unannealed carbon-steel castings. But even in the latter these beams are often lacking from many parts of the specimen and even from many fields of the given section, so that some parts of the specimen may be like his Fig. 1 and others like his Fig. 3. Would he kindly tell us whether

Mr. Howe. these Wiedmannstattian beams are wholly lacking from such vanadium steel, or whether they are merely less common, and if the latter, can he get any notion of about how much less common they are?

Mr. Fay. MR. HENRY FAY (*by letter*).—I have read with extreme interest the paper of Mr. Norris on the effect of vanadium on steel, and believe the idea of presenting papers of this kind an excellent one. There is, however, one important thing missing in this paper and that is the references to the sources of information. Those of us who are in the business of transmitting knowledge to others are often asked for the original papers on which our statements are founded, and it would be a great help if we knew how much of this paper could be traced to other sources than Mr. Norris' extended experience.

Much of the literature of vanadium steels is confusing because the steels referred to have also contained chromium and it is very difficult to believe that vanadium confers all of the wonderful properties and chromium none. It seems to me that we should make a sharp distinction between vanadium steels and chrome-vanadium steels, and if Mr. Norris can help us in this line he will be doing a great service.

Mr. Norris. MR. NORRIS (*Author's closure by letter*).—In reply to Mr. Howe, I regret that I cannot supply the Brinell hardness numbers for all of the tests for Table I in my paper, as hardness measurements were made only on the $1\frac{1}{8}$-in. round bars at the head of the table. These hardness numbers are as follows:

TREATMENT	BRINELL HARDNESS.
As rolled	228
Annealed 800° C.	187
Oil Q. and T., 900/600° C.	241
Oil Q. and T., 875/600° C.	...
Water Q. and T., 845/600° C.	248
Water Q. and T., 845/565° C.	274
Water Q. and T., 845/540° C.	272
Water Q. and T., 845/510° C.	282
Water Q. and T., 845/455° C.	319
Water Q. and T., 845/400° C.	351

In general the Wiedmannstattian structure is absent in vanadium-steel castings. It may occasionally occur, but I believe such occurrence would be exceptional.

Mr. Norris. In reply to Mr. Fay's criticism of lack of definite references to original sources of information made use of, I can only plead in extenuation the very short and much interrupted time in which I had to prepare my contribution to this topical discussion. The most important of these references to vanadium steels I have given in the following list. I have included in this list only references to simple or ternary vanadium steel.

It is not my intention, nor I believe of any competent person, to claim that vanadium confers all the wonderful properties on steel and chromium or other alloying metals none. The presence of vanadium increases the properties of such steels very greatly, and this increase is brought about by a relatively minute amount of vanadium, usually only about 0.15 per cent.

REFERENCES:

J. O. Arnold, Reports to The Alloy Syndicate, Ltd., and The New Vanadium Alloy, Ltd., London, 1900 and 1902.

J. O. Arnold, "Record of the Discovery of the Influence of Vanadium on Steel," *Journal*, Iron and Steel Inst., 1915; comprises above reports.

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L. Guillet, "Etude Industrielle des les Alliages Metallique," 1906.

Paul Putz, "Der Einfluss des Vanadins auf Eisen und Stahl," *Metallurgie*, 1906.

Walter Geisen, "The Special Steels in Theory and Practice," *Journal*, Iron and Steel Inst., Carnegie Fellowship Memoirs, 1909.

A. M. Portevin, "Contribution to the Study of the Special Ternary Steels," *Journal*, Iron and Steel Inst., Carnegie Fellowship Memoirs, 1909.

Andrew McWilliam and Ernest Barnes, "Influence of Vanadium on Steels of Varying Carbon Contents," *Journal*, Iron and Steel Inst., 1911.

W. H. Hatfield, "Influence of Vanadium upon the Physical Properties of Cast Iron," *Journal*, Iron and Steel Inst., 1911.

DISCUSSION ON CHROME VANADIUM.

Mr. Abbott. MR. ABBOTT.—Mr. Griffiths states that chrome-vanadium steel stands forth unmistakably as a master alloy. I will produce in nine times out of ten, as good or better physical results with a plain nickel or a chrome-nickel steel, than can be done with the ordinary chrome-vanadium steel.

Mr. Darke. MR. J. M. DARKE.—I should like to inquire as to the size of the test pieces used by Mr. Griffiths.

Mr. Griffiths. MR. F. J. GRIFFITHS.—The standard 2-in. test piece recommended by this Society was used in the experiments described. The specimens are not threaded, however, and inasmuch as we use wedge grips in our testing machine, the total length of the test piece, including the turned and unturned portions, is approximately 18 in.

In response to the query about the specific uses to which certain alloy steels may be put and the fact that their particular qualities seriously limit their complete usefulness in fields outside of those to which they are particularly adapted, I will state that at our plant, where we are making all grades of alloy steel, we do not have this feeling towards chrome vanadium. We consider, I might say, that chrome vanadium is not designed to be best for certain specific uses but has rather been proved to be specifically best for all uses. As we have said elsewhere, for every general commercial purpose there is a suitable type of chrome-vanadium steel. I have no desire to make these statements seem too broad to be credited, but I think there need be no uncertainty in regard to the reasonable grounds which they are intended to cover. Of course I do not mean to say that it is advisable to use chrome-vanadium steel in places where the properties of ordinary carbon steel are sufficient to ensure a satisfactory performance of the required duty, nor am I speaking of such ultra-special requirements as are necessary in the case, for example, of self-hardening high-speed steel.

GENERAL DISCUSSION.

Mr. Howe. MR. HOWE (*by letter*).—These papers as a whole seem to bear out an opinion which I have long held, that the chief merit of the pearlitic alloy steels over plain carbon steel is due to the sluggishness of coalescence caused by the alloying element, whether manganese, nickel, chromium, or vanadium. This leads to the presence of sorbite instead of pearlite. These alloy steels, as for instance chrome-vanadium (page 41), can be worked at a relatively high temperature without being unduly coarsened, the coarsening being simply one form of coalescence.

Again vanadium steel castings can be given a higher annealing **Mr. Howe.** temperature than those of carbon steel (page 24), thus effacing better the solidificational heterogeneousness, without undue coarsening. In general, the finer the structure the better the quality, irresoluble sorbite being best, then sorbitic pearlite, then lamellar pearlite, then divorced pearlite, and last of all granular pearlite. So, too, with the pro-eutectoid element present, the ferrite or cementite. The finer it is the better is the quality.

A striking example of this advantage is its enabling us to lessen the cooling stresses, while still getting the sorbitic state by restricting the coalescence. Thus an oil cooling or even an air cooling may give, in an alloy steel, the fineness of structure which in the case of carbon steel could be had only by water quenching, with its attendant severe stresses and danger of cracking. And after quenching or air cooling, such stresses as have been caused can be relieved more completely than in carbon steel without losing the irresoluble sorbitic state, because the alloy steel can be drawn at a higher temperature, and thus with more complete relief of stress, than a carbon steel, for like degree of coalescence of the ferrite and the cementite.

This line of thought gives force to the words of Mr. Abbott in this discussion, that some of the merit of the vanadium steel forgings of Mr. Norris' Table I must be referred to their high manganese content.

One is inclined to suspect that, in the pearlitic state, these alloy steels are in large part interchangeable, and that the true line of inquiry for learning the relative value of the alloying elements is to trace their relative quantitative effect in restraining coalescence under like conditions.

ANNEALING TEMPERATURES AND GRAIN GROWTH.[1]

By D. J. McAdam, Jr.

SUMMARY.

This paper describes some observations and experiments on "Admiralty metal" and on cold-wrought ingot iron.

The structures of improperly annealed tubes are illustrated and described. Results of annealing experiments are also given.

The ingot iron was given a preliminary treatment to produce small grain size. It was then reduced in thickness by varying amounts and annealed at temperatures varying from 900 to 1570° F. Photomicrographs of typical structures are shown. The results are summarized in a table.

For ingot iron under these conditions, there is apparently a "recrystallization range" of temperature, extending from about 900 to 1475° F. In this recrystallization range, the grain size for any definite amount of previous deformation seems to be independent of the temperature, and to depend only on the amount of deformation. The smaller the deformation, however, the higher the temperature necessary to produce recrystallization.

There is also a "coalescence range" extending from about 1475° F. to the Ac3 point. Within this range there is coalescence of the "active" grains as well as recrystallization of cold wrought metal.

The effects of impurities are also discussed.

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[1] This paper is presented by permission of the Secretary of the Navy.—Ed.

ANNEALING TEMPERATURES AND GRAIN GROWTH.[1]

By D. J. McAdam, Jr.

The following paper describes the microstructures produced in metals by various degrees of cold working, followed by annealing at various temperatures. These experiments were carried out in connection with certain tests at the U. S. Naval Engineering Experiment Station, Annapolis, Md.

In these experiments, the writer claims no originality of method or theory. The subject of cold working and subsequent recrystallization and grain growth, has been studied during the last few years by a number of investigators and much light has been thrown on the behavior of metals under such conditions. It seemed possible, however, that a description of additional experiments along this line might be of sufficient value to be put on record. For this reason a description of certain experiments, with photomicrographs of the typical structures produced, has been incorporated in the present paper. An incomplete bibliography of the literature on this subject is given at the end of the paper.

The writer's attention was drawn to this subject as a result of observation of the microstructure of certain condenser tubes. It is well known that such material is produced by repeated cold drawing followed each time by annealing. In this process, the varying amounts of reduction in the different passes, followed by varying temperatures and time of annealing, give possibilities of wide variation in the final microstructure of the tubes.

The tubes in question were of the composition known as "Admiralty metal." They had about 70 per cent of copper, 29 per cent of zinc and 1 per cent of tin. The microstructure of such material consists of but one constituent, the "alpha" solid solution.

Figs. 1 and 2, Plate I, show respectively the structure of a good and of a defective condenser tube as received for examina-

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[1] This paper is presented by permission of the Secretary of the Navy.—Ed.

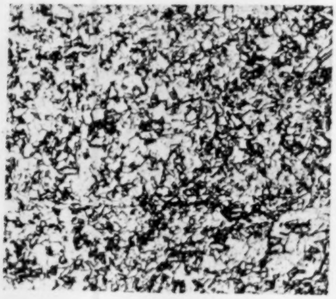

Fig. 1.—Good Tube, as Received.

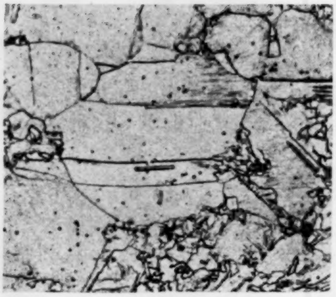

Fig. 2.—Defective Tube, as Received.

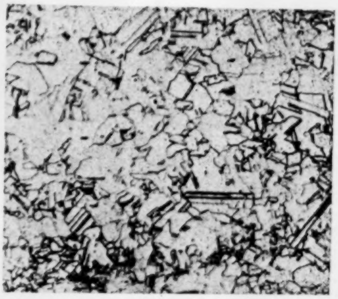

Fig. 3.—Defective Tube, Annealed 30 Min. at 750° F.

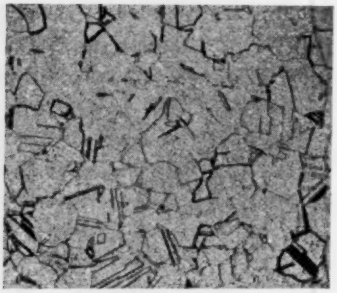

Fig. 4.—Good Tube, Annealed 30 Min. at 1050° F.

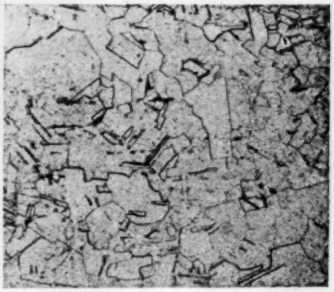

Fig. 5.—Defective Tube, Annealed 30 Min. at 1050° F.

Plate I.—Condenser Tubes, Admiralty Metal (×100).

tion. The defective tube had developed longitudinal cracks near the ends. The structure of the good tube consists of fine, equiaxed grains of the "alpha" solid solution. The structure of the defective tube consists of two kinds of grains: (1) large distorted grains showing the effects of cold working, and (2) much smaller grains occurring chiefly on the boundaries of the coarse grains. Evidently this tube at the end of the annealing was still in process of recrystallization. The small grains seen in Fig. 2 are the active, developing grains, while the large distorted grains are the inert, broken-down grains.

Fig. 3, Plate I, shows the effect of annealing the defective tube for 30 minutes at 750° F. It can be seen that the small grains have increased at the expense of the coarse grains, thus nearly obliterating the original distorted structure. Figs. 4 and 5 show the effect of annealing both tubes at 1050° F. for 30 minutes. Recrystallization and coalescence has taken place, so that both tubes now consist of moderate sized, equiaxed grains.

The above illustrations show the value of the microscope in checking the processes of manufacture of such cold-worked material and determining when the annealing has been properly done. After the above experiments had been carried out, the writer's attention was called to the exhaustive work of Professor C. H. Mathewson on "The Recrystallization of Cold-Worked Alpha Brass on Annealing."[1] A study of the results and conclusions there given was found of great help in additional work along this line.

EXPERIMENTS WITH INGOT IRON.

For studying the laws of deformation, recrystallization and grain growth, various metals were used by the writer. The results obtained with ingot iron, however, are the only ones available at the present time. The ingot iron was obtained from the American Rolling Mill Co., in the form of 1-in. square bars.

For applying cold work in a fairly definite degree a 300,000-lb. testing machine was used in compression. The amount of cold work has been expressed in percentage reduction in thickness of the material.

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[1] *Bulletin*, Am. Inst. Min. Engrs., January, 1916.

Since the material as received was rather coarse grained, and since Z. Jeffries[1] has pointed out that fine-grain material is usually most suitable for studying the laws of grain growth, the ingot iron was given a preliminary treatment for the purpose of producing a fine-grain structure. It was reduced about 80 per cent in thickness and annealed for 30 minutes at 1300° F. The structure thus produced is practically as shown in Fig. 6, Plate II.

The plates of metal, about $\frac{1}{4}$ in. thick, were now given various reductions in thickness by the method described above. The reductions were from 0 to 60 per cent in intervals of 5 per cent.

The various specimens were then cut into smaller pieces and annealed at the temperatures and for the times given in Plates II to IX. For annealing the specimens an electrically heated muffle furnace was used. The furnace was brought to the desired temperature and the specimens were then inserted. Since the specimens were small, the time required to reach the desired temperature was not over ten minutes in each case. The microstructures thus produced are illustrated in these figures at a magnification of 100.

The phenomena as regards recrystallization are summarized in Table I. In this table, the letters *I*, *G*, *R* and *C* refer to the condition of the metal at the end of the annealing process. Metal represented by the letter *I* is still in the "inert" or uncrystallized condition. The letter *G* represents the "germinant" condition or partially recrystallized metal; *R* represents the completely recrystallized condition, and *C* represents the "coalescent" condition which was found in this metal at a temperature above 1475° F.

These conditions will now be described somewhat in detail. In Plate II, Figs. 6 to 9, inclusive, show that the metal is in its inert, unrecrystallized condition. Up to and including 25 per cent reduction, the annealing at 900° F. for $8\frac{1}{2}$ hours has produced no apparent effect. In the specimens that have received greater reduction, as shown in Figs. 10 and 11, examination will show that many new grains have formed in the midst of the elongated, distorted grains. The recrystallization, however, was evidently incomplete at the end of this annealing period. In regard to Fig. 10 it may be said that it does not represent the average structure of that specimen of metal, but shows the structure found in certain regions near the outer surface. In these experiments it was frequently noticed that recrystallization was more advanced near the outer surface than in the interior of a specimen.

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[1] *Bulletin*, Am. Inst. Min. Engrs., May, 1916; also, "Grain Growth Phenomena in Metals," *Ibid.*, November, 1916.

Annealing at a slightly higher temperature, and for a longer time, as shown in Plate III, has produced much more conspicuous results. The first two specimens, Figs. 12 and 13, show no evidence of recrystallization. In Figs. 14, 15 and 16, however, new "germinant" grains, as Henry M. Howe[1] has called them, are visible in the midst of the "inert" grains. In Fig. 16, the recrystallization appears to be nearly complete. In the more highly reduced specimens, shown in Fig. 17, the recrystallization is complete.

TABLE I.—SUMMARY OF RESULTS OBTAINED WITH INGOT IRON.

Temperature and Time of Annealing.		Reduction in Thickness, per cent.								
Deg. Fahr.	Hours.	5	10	15	20	25	30	40	50	60
900	8½	*I*	*I*[1]	*I*	*I*	*G*	*G*	..	*G*[1]	*G*
950	16	*I*	*I*[1]	*I*	*G*	*G*	*G*	*R*	*R*[1]	..
1000	8½	*I*	*I*	*G*	*G*	*G*	*R*	*R*[1]	..	..
1050	2	*I*	*I*	*G*	*G*	*R*	..	..	*R*	..
1100	9	*I*	*G*	*G*	*R*	*R*	*R*[1]	*R*	..	..
1275	4	*I*	*G*	*G*	*R*	*R*[1]	*R*	*R*[1]	*R*[1]	*R*
1450	2¾	*I*	*G*	*R*	*R*	*R*[1]	*R*	*R*[1]	*R*[1]	*R*
1570	2½	*C*	*C*	*C*	*C*	..	*C*	*C*[1]	..	*C*

I...........Metal in "inert" or uncrystallized condition.
G...........Metal in "germinant" or partially recrystallized condition.
R...........Metal in completely recrystallized condition.
C...........Metal in "coalescent" condition (above 1475° F.).

[1] Micrographs of these specimens are not shown. All others are shown in Plates II–IX, inclusive.

Annealing at a still higher temperature, 1000° F., as shown in Plate IV, has caused germination of some grains in the material that has received 15 per cent reduction. Recrystallization, however, is not complete below about 30 per cent reduction.

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[1] "On Grain Growth," *Bulletin*, Am. Inst. Min. Engrs., December, 1916.

In the iron that was annealed at 1050° F., though the time of annealing was short, the recrystallization of the 15 per cent material was nearly complete. Fig. 26, Plate V, shows that large "germinant" grains have absorbed most of the small "inert" grains. The recrystallization was also incomplete in the iron that had received 20 per cent reduction. For greater reductions, the recrystallization is complete.

Annealing at 1100° F. has started recrystallization in the material that was reduced 10 per cent. Below 20 per cent reduction, however, the recrystallization is incomplete. The influence of non-metallic inclusions is especially noticeable in Fig. 32, Plate VI, and in Fig. 26 of Plate V. The recrystallization has proceeded most freely in regions fairly free from these inclusions.

Annealing for 4 hours at 1275° F., as shown in Figs. 36 to 41, Plate VII, has produced a more advanced stage of recrystallization especially noticeable in the specimens that received 10 and 15 per cent reductions. Annealing at 1450° F., as shown in Figs. 43 and 44, Plate VIII, has nearly completed the recrystallization of the specimen that had received 10 per cent reduction, and has entirely completed the recrystallization of the 15-per-cent specimen.

Recrystallization Range.—It is worthy of note that the above described annealings, at temperatures from 950 to 1450° F., inclusive, have produced approximately the same grain size in metal having the same reduction of area. For example, Figs. 33, 39 and 45 show approximately the same grain size. As shown by the corresponding photomicrographs, the grain size for each degree of reduction beyond 30 per cent seems to be nearly independent of the temperature of annealing, for temperatures between about 950 and 1475° F. The only apparent effect of varying the temperature from 950 to 1475° is to cause recrystallization at progressively less amounts of deformation.

Deformed ingot iron, therefore, between about 900 and 1475° F. behaves as if it contained nuclei or germs of recrystallization, the number of these nuclei depending on the degree of deformation of the metal. Slightly deformed iron contains few nuclei, while iron that has received a large amount of deformation contains many nuclei.

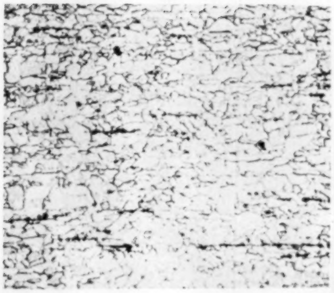

FIG. 6.—Reduction 5 per cent.

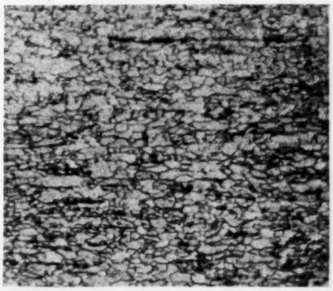

FIG. 7.—Reduction 15 per cent.

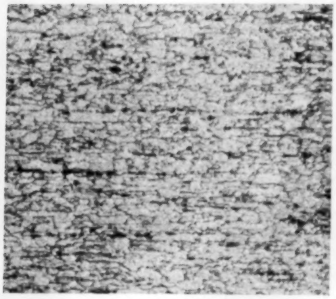

FIG. 8.—Reduction 20 per cent.

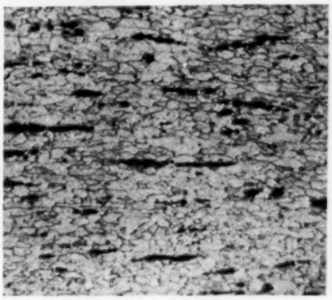

FIG. 9.—Reduction 25 per cent.

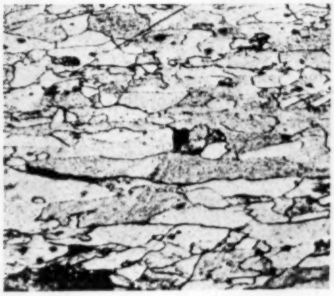

FIG. 10.—Reduction 30 per cent.

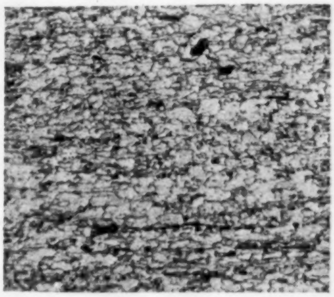

FIG. 11.—Reduction 60 per cent.

PLATE II.—INGOT IRON, ANNEALED 8½ HOURS AT 900° F. (×100).

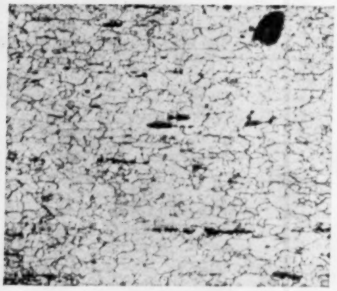

FIG. 12.—Reduction 5 per cent.

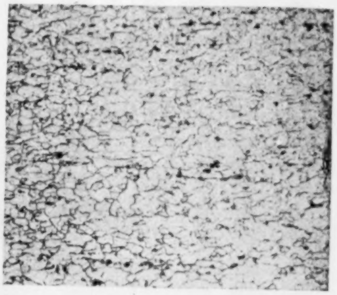

FIG. 13.—Reduction 15 per cent.

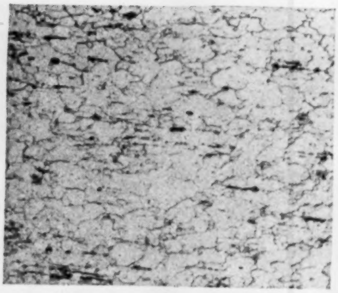

FIG. 14.—Reduction 20 per cent.

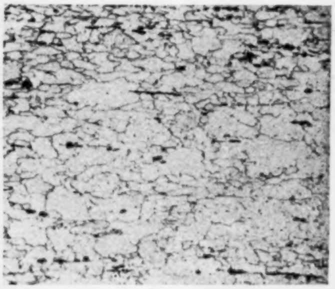

FIG. 15.—Reduction 25 per cent.

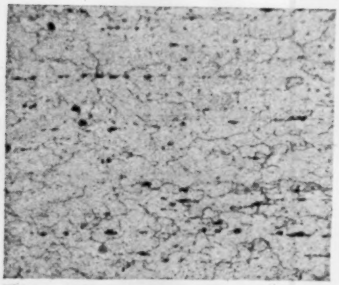

FIG. 16.—Reduction 30 per cent.

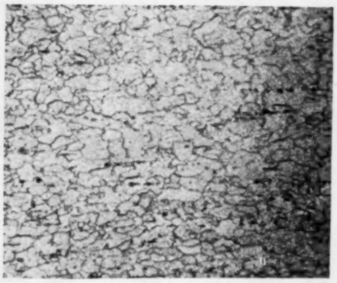

FIG. 17.—Reduction 40 per cent.

PLATE III.—INGOT IRON, ANNEALED 16 HOURS AT 950° F. (×100).

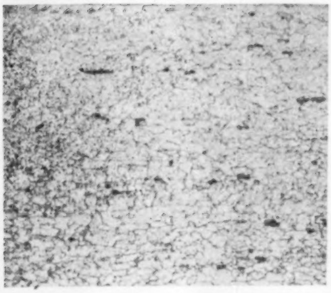

FIG. 18.—Reduction 5 per cent.

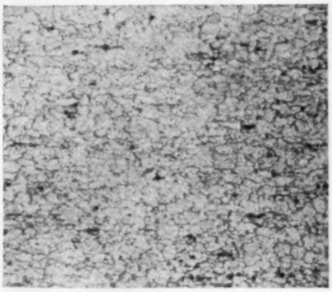

FIG. 19.—Reduction 10 per cent.

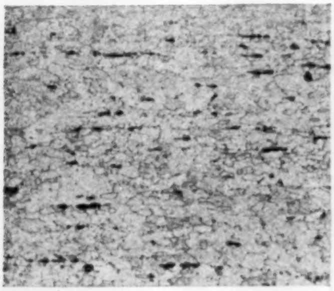

FIG. 20.—Reduction 15 per cent.

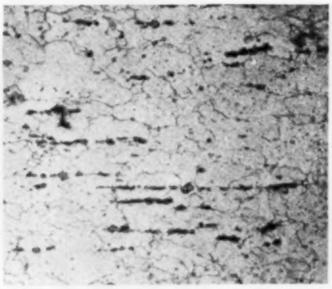

FIG. 21.—Reduction 20 per cent.

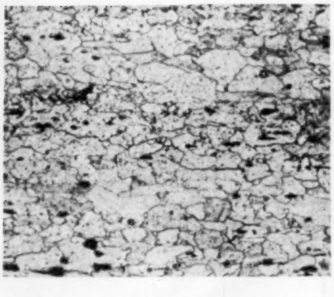

FIG. 22.—Reduction 25 per cent.

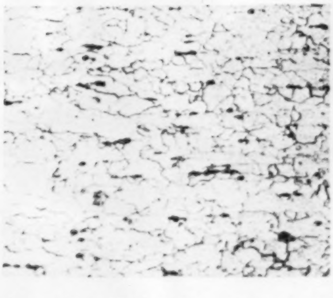

FIG. 23.—Reduction 30 per cent.

PLATE IV.—INGOT IRON, ANNEALED 8½ HOURS AT 1000° F. (×100).

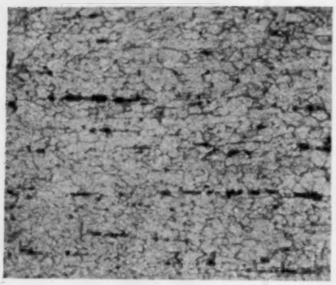

Fig. 24.—Reduction 5 per cent.

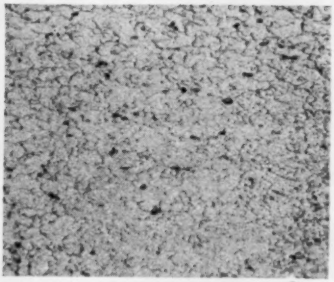

Fig. 25.—Reduction 10 per cent.

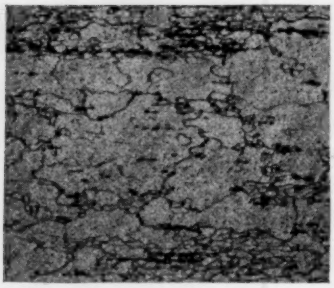

Fig. 26.—Reduction 15 per cent.

Fig. 27.—Reduction 20 per cent.

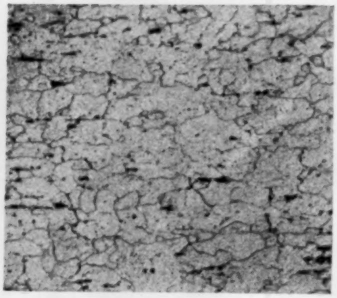

Fig. 28.—Reduction 25 per cent.

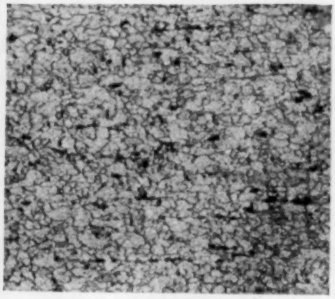

Fig. 29.—Reduction 50 per cent.

Plate V.—Ingot Iron, Annealed 2 Hours at 1050° F. (×100).

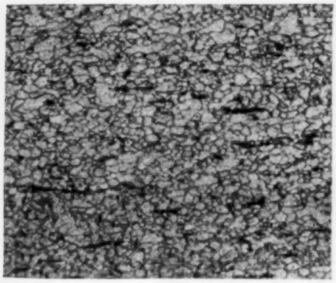

FIG. 30.—Reduction 5 per cent.

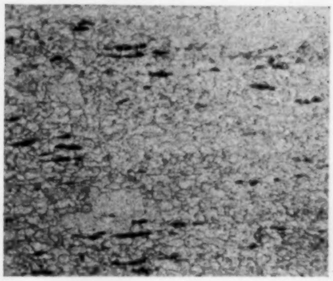

FIG. 31.—Reduction 10 per cent.

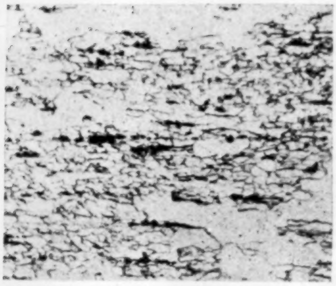

FIG. 32.—Reduction 15 per cent.

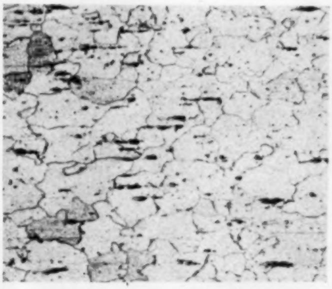

FIG. 33.—Reduction 20 per cent.

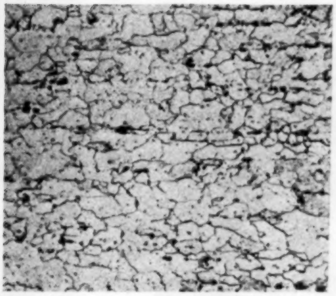

FIG. 34.—Reduction 25 per cent.

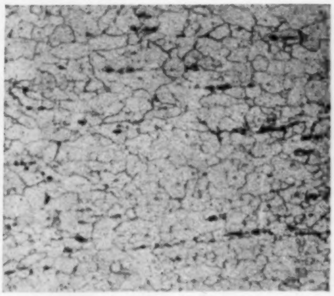

FIG. 35.—Reduction 40 per cent.

PLATE VI.—INGOT IRON, ANNEALED 9 HOURS AT 1100° F. (×100).

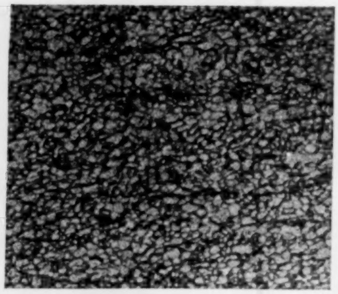

Fig. 36.—Reduction 5 per cent.

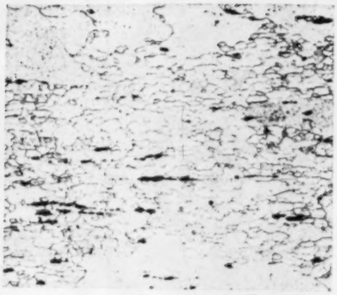

Fig. 37.—Reduction 10 per cent.

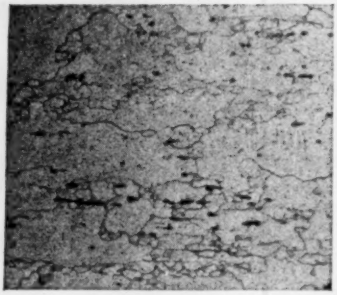

Fig. 38.—Reduction 15 per cent.

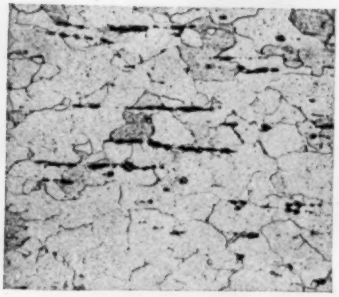

Fig. 39.—Reduction 20 per cent.

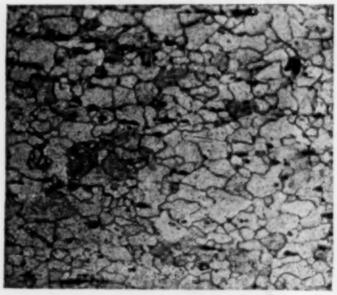

Fig. 40.—Reduction 30 per cent.

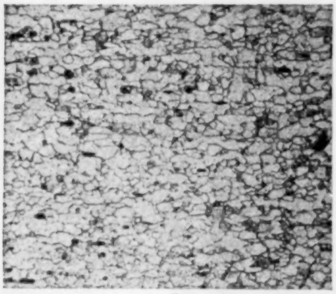

Fig. 41.—Reduction 60 per cent.

Plate VII.—Ingot Iron, Annealed 4 Hours at 1275° F. (×100).

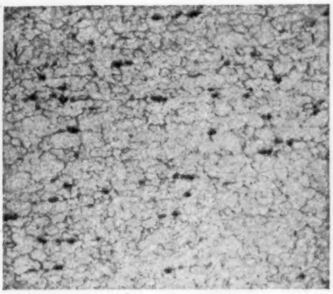

FIG. 42.—Reduction 5 per cent.

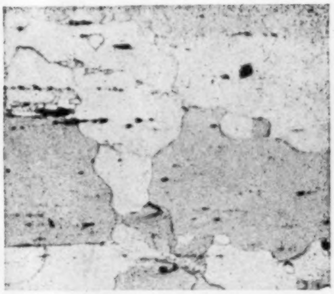

FIG. 43.—Reduction 10 per cent.

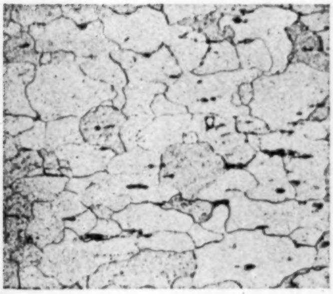

FIG. 44.—Reduction 15 per cent.

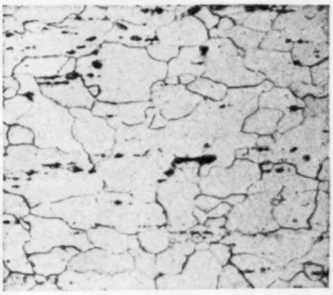

FIG. 45.—Reduction 20 per cent.

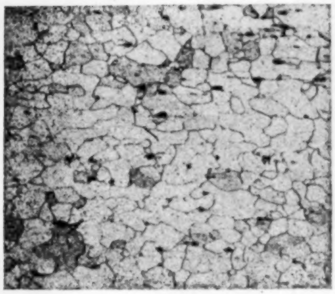

FIG. 46.—Reduction 30 per cent.

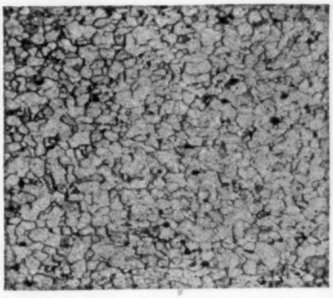

FIG. 47.—Reduction 60 per cent.

PLATE VIII.—INGOT IRON, ANNEALED 2¾ HOURS AT 1450° F. (×100).

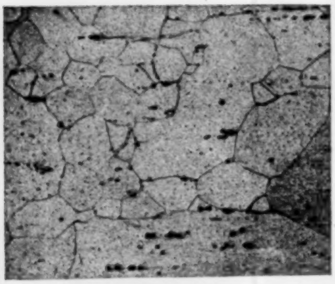

Fig. 48.—Reduction 5 per cent.

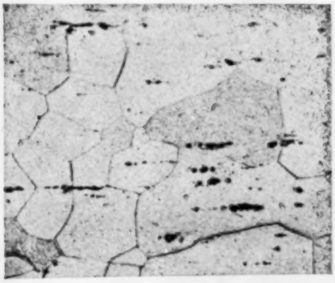

Fig. 49.—Reduction 10 per cent.

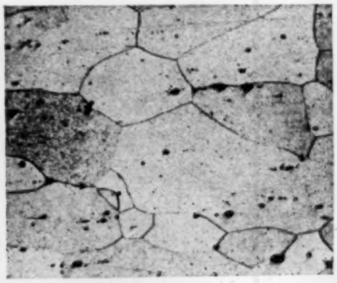

Fig. 50.—Reduction 15 per cent.

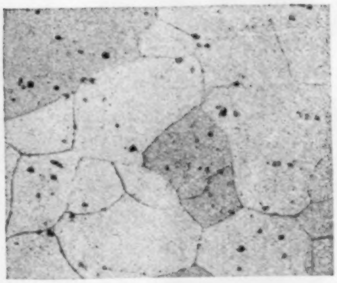

Fig. 51.—Reduction 20 per cent.

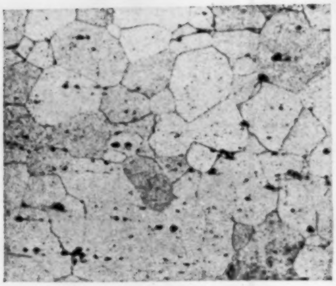

Fig. 52.—Reduction 30 per cent.

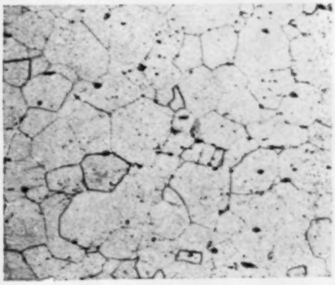

Fig. 53.—Reduction 60 per cent.

Plate IX.—Ingot Iron, Annealed 2½ Hours at 1570° F. (×100).

Variations of grain size in recrystallized ingot iron (and probably also in other metals within certain temperature limits) seem to depend merely on the number of nuclei. Few nuclei mean large grain size; many nuclei mean small grain size in the recrystallized metal. Progressively higher temperatures, therefore within the above defined limits, will not cause increased grain size for the same amount of deformation, but will permit recrystallization of progressively less deformed material.

"Coalescence" Range.—The range of temperature between about 900 and 1475° F., therefore, may be called the recrystallization range for ingot iron. Experiments have shown that within this range, little or no coalescence of the "active" grains is caused by annealing for seven hours or less. Above about 1475°, however, the phenomena of coalescence of the active grains begin to be noticed. Annealing for seven hours at 1500° was found to cause slight coalescence. Annealing for only 2½ hours at 1570° F., as shown in Figs. 48 to 53, Plate IX, has caused grain growth for all degrees of reduction. A comparison of Plates VIII and IX will make this evident. The range of temperature between about 1475° F. and the Ac3 transformation point, therefore, may be called the "coalescence" range for ngot iron.

Effect of Non-Metallic Inclusions and of Slight Variation in Composition.

There is no doubt that non-metallic inclusions exert a retarding effect on the recrystallization of ingot iron. In addition to this retarding effect, these "sonims" probably exert a considerable effect on the number of "germinative nuclei." It is possible that such foreign objects may increase the number of such nuclei, thus producing smaller grain size in the completely recrystallized material. Whatever may be the theoretical explanation, it may be said that the practical effect of such "impurities" is as if they had merely altered the number of nuclei of recrystallization. This statement applies to soluble as well as insoluble impurities.

Conclusions.

For cold-wrought ingot iron the "recrystallization range" of temperature extends from about 900 to 1475° F. Within this range the metal, annealed as above described, behaves as if it contained recrystallization nuclei. The number of these nuclei seems to depend merely on the amount of deformation of the metal. Small deformation produces few nuclei, large deformation produces many nuclei. The grain size of the recrystallized material, therefore, after any definite degree of deformation, is independent of the temperature, and depends only on the amount of previous deformation. The smaller the deformation, however, the higher the annealing temperature necessary to produce recrystallization.

From about 1475° F. to the Ac3 transformation point, there is for iron a "coalescence range," in which not only recrystallization of cold wrought metal takes place, but also coalescence of the "active" grains. In the "coalescence range," therefore, there is grain growth after any degree of deformation.

These conclusions seem to be established for ingot iron of originally small grain size, annealed as above described. To determine how far they apply to other material, additional experiments would be necessary.

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Z. Jeffries, "Grain Growth Phenomena in Metals," *Bulletin*, Am. Inst. Min. Engrs., November, 1916.

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DISCUSSION.

MR. HENRY M. HOWE.—This paper is extremely welcome as giving results of the wealth of systematic observations. **Mr. Howe.**

The most important contribution is the evidence of the marked acceleration of grain growth above about 700° C.

Table I conforms to Chappell's law[1] that the re-crystallization temperature is inversely as the prior plastic deformation.

Most of the grain growths reported in the paper appear to be of the normal slow type as distinguished from the injurious gross coarsening to which plastically deformed metal is subject. According to the hypothesis of Professor Jeffries such coarsening occurs at the contact of inert and germinant grains, the germinant grain absorbing the inert one readily, and gaining thereby in absorptive power because of its increased size. Any given grain becomes germinant on reaching a certain germinative temperature, which is the lower the greater the prior deformation has been. On the other hand, the rapidity of grain growth here, as under other conditions, increases with the temperature.

An analysis of this hypothesis leads us to infer that, in the absence of a strain gradient, coarsening requires the coexistence, first, of such contact between germinant and inert grains, the former being necessarily the hotter of the two in the absence of a strain gradient; and second, of a temperature movement past the germinant temperature for the strain in question, slow enough to enable certain individual grains to retain for a long period their advantage of being germinant while in contact with inert ones, the length needed for this period being inversely as the prior strain.

The presence of a strain gradient may either increase or decrease the rate of coarsening. But it increases the liability to coarsening, because, whereas in its absence there is only one temperature at which coarsening can occur, and whereas this temperature is likely to be passed through in the heating and cooling too slowly to permit much growth to occur at it, the

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[1] *Journal*, Iron and Steel Inst., Vol. I, p. 473, and Fig. 9, Plate XIV (1914).

Mr. Howe. presence of a strain gradient substitutes for such a single temperature a range of temperature, at any point in which a properly slow temperature movement should induce coarsening.

The temperature movement in most annealings is of the type shown in the accompanying figure. It is only during the part of the annealing between *A* and *B* that the temperature movement is slow enough to give long exposure to any given single temperature, such as the germinative temperature in a uniformly strained sheet. But if there is a strain gradient covering a wide range of strain, and causing thus a wide range of temperature every point in which is the germinative temperature of part of the sheet, then the chance that the part *AB* of this annealing will occur somewhere in this germinative range is far greater.

In view of this consideration, it may well be that it is Mr. McAdam's careful prevention of both thermal and strain gradients that has prevented rapid coarsening in his experiments. There is indeed a suggestion that his 10-per-cent reduction sets up a germinative temperature near 788° C. (1450° F.) because the size of the largest grain in the micrograph of the 10-per-cent 788° specimen, about 35,500 μ^2, is of the order common in rapid grain coarsening. We may infer that the germinative temperature for this reduction is so slightly below 788° that it passed across the specimen so slowly as to give the grains which it reached first ample time to feed on the still inert ones which it had not yet reached, and thus enabled them to gain a predominating size and because of that size to continue their conquests.

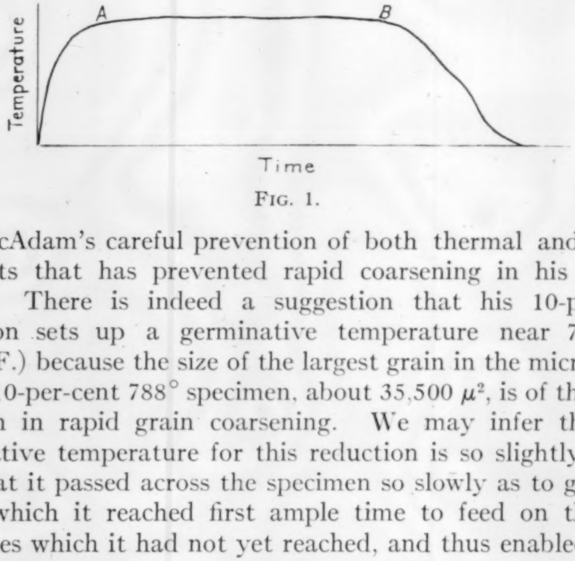

FIG. 1.

The very considerable coarsening of the 15-per-cent 788° specimen is in harmony with this, because the corresponding somewhat lower germinative temperature, though it would be

Mr. Howe.

passed through more rapidly than that of the 10-per-cent specimen, might yet be passed through slowly enough to give a fair opportunity for coarsening.

Without the Jeffries hypothesis, the interpretation of such phenomena is often hopelessly difficult.

Should not the assertion on page 74 that between the limits of 482° and 788° C. (900° and 1450° F.) the grain size is independent of the temperature, be limited in accordance with the corresponding assertion on page 64, to reductions in excess of 30 per cent? One fancies so, because 20-per-cent reduction seems to be followed by material growth between 482° and 565°, and by still more growth by the time 788° C. is reached, and the 10-per-cent reduction by growth at 690° and by much more at 788°. Even with this limitation one hesitates to accept this assertion without quantitative measurements. Thus there is certainly a suggestion of growth between 482° and 788° after the 40, 50, and 60-per-cent reductions in the excellent photographs which Mr. McAdam has most kindly given me. Again, the rather irregular variations in the length of exposure, falling for instance from 16 hours at 482° to 2¾ hours at 788°, might well mask the influence of higher temperature in hastening growth.

Finally the great coarsening induced by Sauveur[1] and by Chappell[2] at and about 650° shows that in the presence of a strain gradient, coarsening may be rapid in this range of temperature.

Mr. Campbell.

MR. WILLIAM CAMPBELL.—This paper on annealing temperatures and grain growth by Mr. McAdam is of great interest, in that it tends to tie up the results found by Mathewson in the case of alpha brass, by Jeffries and others in the case of non-ferrous metals and alloys, with the grain growth in iron so admirably shown by Sauveur and discussed by Howe.

It shows that iron after straining tends to recrystallize just as any other metal or alloy which is a solid solution: that there is a distinct relation between the amount of cold work and annealing temperature. In the past many people have thought that iron was peculiar, because they did not realize there were

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[1] "The Metallurgy and Heat Treatment of Iron and Steel," Second Edition, p. 265.

[2] *Journal*, Iron and Steel Institute, 1914, Vol. I, Pl. XLV, after p. 496.

Mr. Campbell. two kinds of recrystallization: (1) that due to grain growth after strain; (2) that due to heating above the upper critical point Ac3. We are now beginning to understand that this so-called "Stead's brittleness" is not a freak but something which occurs to a greater or lesser extent in all strained metal.

The effect of impurities on grain growth is of great importance. We have long known that strained wrought iron is not as prone to recrystallize in heating as is low-carbon steel. The presence of the entangled slag is one of the greatest factors. Then, again, as we increase the carbon in steel, we increase the pearlite, which tends to decrease the tendency to recrystallize. It is now well known that the presence of impurities in drawn tungsten wire tends to prevent the formation of extremely coarse crystals in a filament after use, known as "offsetting."

In 1899 I started work on the structure of metals, the effect of work and of annealing, and soon found cases in which rolled metal behaved abnormally. Samples of sheet lead on annealing showed one or two enormous crystals, while the rest of the metal was extremely fine grained. Strips of zinc showed abnormal recrystallization, some samples being composed of such large crystals that one occupied the entire cross-section and resembled some of Ruder's enormous crystals in silicon steel. Similar results were found in samples of tie-rods from reverberatory furnaces where bars 1½ in. in diameter showed but a single crystal in the cross-section.

More recently, in examining the crystallization of brass cartridge cases at different stages in the process of manufacture, examples of abnormal grain growth were found in the head. May not these have some definite connection with some of the cases of season cracking?

Many instances of abnormal cases of recrystallization have been found in copper. In some work which I started about four years ago some of my copper specimens showed enormous crystals while others treated in the same way did not. After much speculation as to the cause it suddenly dawned on me that every case of coarse recrystallization occurred directly beneath the place where the sample number had been stamped on the specimen.

In regard to the recrystallization of iron, I have been at

Mr. Campbell. work on some very pure iron made by Dr. J. A. Mathews in the crucible in 1901 for his Carnegie Research Scholarship. In order to vary the amount of strain I bent some samples into U shape, getting both tension and compression in the bend. Others I hammered into the form of a wedge. While the work is not yet finished there is every indication that the results will confirm those presented in this paper.

Mr. Jeffries. MR. ZAY JEFFRIES (*by letter*).—My remarks will relate particularly to the recrystallization of the American ingot iron.

Mr. McAdam's results are very interesting and very valuable, because it was considered for a long time that the formation of coarse grains at certain temperatures of annealing after cold plastic deformation would not take place in iron and steel products containing less than about 0.04 per cent of carbon. For example, Henry M. Howe[1] writes as follows regarding the failure to produce coarsening of the grain structure in these products:

> "It is true that Professor Jeffries' hypothesis explains satisfactorily and without needing this obstruction principle certain other cases of unexpected failure to coarsen, for instance that of my tapered bar of steel of about 0.01 per cent of carbon from the American Rolling Mill Co. This, after straining with tensile stresses varying from 40,640 to 49,000 lb. per sq. in., failed to coarsen noticeably in a 22-hour exposure to 680°. Because the yield point of this steel is only about 20,000 lb. per sq. in., even the least of my stresses may well have caused a degree of deformation so great that the corresponding germinative temperature was far below 680°, so that, in heating up to 680°, the germinative temperature of each of the various parts was passed through too rapidly to lead to material coarsening.
>
> "Yet this explanation hardly applies to Professor Sauveur's failure to coarsen his bent specimen in a 7-hour exposure to 650° in case the carbon content is less than 0.04 per cent, for here there should be layers near the neutral axis in which the plastic deformation should vary progressively from nil to far above that for which the

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[1] "On Grain Growth," *Bulletin*, Am. Inst. Min. Engrs., December, 1916, p. 2116.

Mr. Jeffries.

> germinative temperature is 650°. Somewhere in this progressive series there should be a layer with the deformation for which 650° is the germinative temperature, and this layer should have coarsened at the expense of its less strained neighbors nearer the neutral axis. But here the failure to coarsen may be explained by the suspected presence of gas bubbles, to which such low-carbon steel is so subject."

The writer[1] has expressed the following views concerning this same point:

> "While Professor Sauveur observes that coarse grains did not form in nearly pure iron, it has been the writer's experience that there is nearly always a region in which the grains are larger than those on either side. Sometimes the difference is not great enough to be unmistakably observed in the ordinary microscopic examination, but grain-size determinations usually show the difference. A good example of the grain-growth phenomena in a solid solution comparatively free from foreign bodies will be found in Mathewson and Phillips' micrographs.
>
> "In a given pure metal with a given amount of cold deformation, the grain size before deformation will have a marked effect on the selective grain growth during annealing. If the initial grain size is large the grain fragments formed during annealing will be correspondingly large, and if the initial grain size is small the grain fragments will also be smaller than in the previous case. The latter condition is conducive to the formation of large grains at the germinative temperature. Instead of normalizing the pure iron, if Professor Sauveur had given it an initial treatment so as to produce small grains he would have noticed a more marked recrystallization effect after cold deformation with subsequent annealing. On the other hand, if the grains are initially very large it is possible almost completely to mask the fast growth phenomenon."

It seems to me that the failure to coarsen nearly pure iron in the past has been due to the fact that before applying the

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[1] "Grain-Growth Phenomena in Metals," *Bulletin*, Am. Inst. Min. Engrs., November, 1916, p. 2066.

Mr. Jeffries.

deformation previous to the heating operation the iron and steel products have been subjected to the normalizing treatment. This treatment consists essentially in heating the metal to a temperature above 900° C., followed by furnace cooling. If the amount of carbon is appreciable, say more than 0.04 or 0.05 per cent, the obstruction of the austenite during cooling to 700° or thereabouts is sufficient to render the resulting material rather fine grained. When this comparatively fine grained material is deformed by bending, mechanical working, or by impression with a Brinell machine it is in a condition which is favorable to the formation of large grains when heated to the germinative temperatures, especially if the carbon content is less than about 0.12 per cent. On the other hand, when iron and steel products containing less than about 0.04 per cent of carbon, for example, American ingot iron, are heated to a temperature above 900° and cooled slowly, the ferrite grains are comparatively large. When such a material composed of comparatively large grains is deformed cold and then heated, the conditions are unfavorable to the production of coarse grains. In other words, the exaggerated grain-growth at the germinative temperature is largely masked.

Mr. McAdam, by using a fine grained structure before producing the deformation in the American ingot iron, had conditions favorable for the formation of comparatively large grains at the germinative temperature. Fig. 43, Plate VIII, for example, represents the coarsest grained material of all of the micrographs. There is a very marked contrast in grain size between Figs. 43 and 47. This contrast in grain size is entirely due to the effect at the germinative temperature.

One point of particular interest in Mr. McAdam's results is the very low temperatures at which recrystallization took place. Fig. 11, Plate II, for example, indicates conclusively that recrystallization of American ingot iron was produced in 8½ hours at a temperature of 482° C. This is a very low temperature for the recrystallization of cold-deformed iron.

Plate III indicates conclusively that not only did recrystallization take place freely after a 16-hour sojourn at 510° C., but the exaggerated grain growth due to the germinative temperature laws is quite marked in the region between 20 and 30 per cent reduction in thickness by cold deformation.

Mr. White. MR. A. E. WHITE (*by letter*).—I am sure that science is indebted to the efforts of Mr. McAdam for the splendid contribution he has made to our knowledge of grain growth. It is appreciated that there are a number of factors affecting crystal growth. Some of them are time, temperature, intensity of deformation, type of deformation, composition of metal, etc., and his work will be of great value in assisting in clarifying some of our conceptions concerning the influence of certain of these factors.

In a joint paper presented by White and Wood before this Society at the 1916 Annual Meeting entitled "Recrystallization as a Factor in the Failure of Boiler Tubes"[1] an attempt was made to formulate a time-temperature law governing grain growth for a constant composition, constant type of deformation, constant intensity of deformation, etc. This past winter some work has been done in an attempt to ascertain pressure-time relationships at constant temperatures and pressure-temperature relationships at constant time, but the unsettled conditions of the time and pressure of work have prevented carrying these investigations to completion. It is noted with interest that this phase of the work is being so successfully started by Mr. McAdam.

The writer wonders whether or not a numerical tabulation of the results which could be obtained through crystal count either by the planimetric or intercept methods, would add to the value of the work. It appears to him that such might be the case.

Also the writer wonders what is the composition of the ingot iron used. He is under the impression that it contains less than 0.05 per cent of carbon. If such is the case, the results presented are more or less at variance with conditions stated by one of our eminent scientists, who has given the impression that iron with such a low carbon content does not suffer any appreciable crystal growth. The writer has had a number of experiments made showing the influence of carbon on the crystal growth of deformed steel when exposed at a suitable temperature for a suitable time. He has found crystal growth in practically carbonless iron, although the percentage of growth was not as

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 80 (1916).

Mr. White.

great as in metal containing from 0.11 to 0.15 per cent of carbon. On the assumption that this ingot iron is very low carbon stock he is interested in noting that Mr. McAdam checks his results.

Shortly after the publication of the joint paper referred to above, the writer received many inquiries concerning whether or not wrought iron was subject to crystal growth. It is noted with interest that Mr. McAdam shows that inclusions in steel, which may be considered analagous to slag in wrought iron, lowers but does not eliminate the crystallizing tendency. It seems as if this work, therefore, within certain limits, answers this question.

Mr. McAdam.

MR. D. J. MCADAM, JR. (*Author's closure by letter*).—Since most of the comments on this paper have been favorable, I shall review them briefly and discuss at greater length the questions raised by Mr. Howe.

The writer appreciates the suggestions of Mr. White in regard to chemical analysis and grain-size measurements. If sufficient time had been available, these results would have been included in the paper. Grain-size measurements on this material, and on other ingot iron specimens, have recently been made and will be published in full in a later article.

A chemical analysis of the iron, as reported by the chemical laboratory of the U. S. Naval Engineering Experiment Station, gave the following results:

Carbon	0.034 per cent
Manganese	0.01 "
Phosphorus	0.008 "
Sulfur	0.048 "

As stated by Mr. Howe in his comments, thermal and strain gradients were avoided as much as possible in the experiments described in the paper. The results, therefore, show, as nearly as it is practicable, the grain growth of iron that had been uniformly strained to varying degrees and afterward annealed at various temperatures.

Although grain-size measurements had not at that time been made, a careful comparative examination of the entire surfaces of the specimens brought out certain facts which were summarized as carefully as possible in the general conclusions at

Mr. McAdam. the end of the paper. The conclusions in regard to the effect of temperature on grain growth of recrystallized ingot iron may be re-stated substantially as follows:

(*a*) Below about 1475° F. (801° C.), annealing for seven hours or less causes practically no grain coalescence in the recrystallized material. This range of temperature was, therefore, called the "recrystallization range."

(*b*) Above about 1475° F. (801° C.), there is a decided coalescence of the recrystallized grains. This range of temperature was, therefore, called the "coalescence range."

According to these conclusions the temperature - grain size curve for a definite degree of deformation should be divisible into two parts at a point corresponding approximately to a temperature of 1475° F. Below this point the curve should be nearly horizontal (with temperatures as abscissas); above this point the curve should have a decided upward inclination. It would be expected, of course, that these two divisions would form one continuous curve with no sharp break at 1475° F.

Since Mr. Howe did not have the opportunity to examine the specimens themselves, it is perhaps natural that he should be reluctant to accept certain conclusions without grain size measurements. Evidently, however, he has in one respect misunderstood these conclusions. For example, in the next to last paragraph of his remarks he says: "One fancies so, because 20-per-cent reduction seems to be followed by material growth between 482° and 565° . . . and the 10-per-cent reduction by a growth at 690°." In these remarks he is mistakenly comparing the grain size of *recrystallized* with that of *unrecrystallized* material. A reference to Table I of the paper will show that after 20-per-cent reduction recrystallization did not take place at 482° C. (900° F.); moreover, after the 10-per-cent reduction recrystallization did not take place below 593° C. (1100° F.). As stated also in the paper, the above-mentioned conclusions applied only to the *recrystallized* material. The author is glad to have this opportunity to remove this misunderstanding from the minds of perhaps others as well as Mr. Howe.

At the time of the experiments the writer realized that the varying duration of annealing might be the subject of criticism, as in the next to last paragraph of Mr. Howe's remarks. Against

Mr. McAdam.

this criticism, however, it may be said that increased temperature would be expected to cause increased *rapidity* of grain growth. If, therefore, the grain size of recrystallized metal after $2\frac{3}{4}$ hours at 1450° F. (788° C.) is practically the same as that of similar metal after 16 hours at 950° F. (510° C.), the conclusion seems justified that within this temperature range there is practically no coalescence tendency for grains of uniform size and temperature. Moreover, as stated in the paragraph on "Coalescence Range" in the paper, the conclusions were based on additional experiments not illustrated by photomicrographs.

Grain-size measurements by the method of Jeffries, made since this paper was printed, have shown that the above-mentioned conclusions summarized the results as well as could have been expected. The measurements have also brought out additional points of interest to which only brief reference can be made at the present time. As a result of these measurements, the conclusions can now be re-stated in somewhat different form as follows:

(*a*) For ingot iron free from strain gradient and grain-size gradient there is a "recrystallization range" extending from about 900° F. (482° C.) to the lower limit of the so-called "beta range." Within this range there is little or no coalescence tendency for grains of uniform temperature and size.

(*b*) For such ingot iron there is a "coalescence range" corresponding to the so-called "beta range" (the gamma range will not here be considered).

The "alpha range", therefore, is the "recrystallization range"; the "beta range" is the "coalescence range."

In other words, the temperature - grain size curve of such material corresponds closely to an inverted magnetic intensity curve. The coalescence tendency, therefore, seems to be proportional to the profound change in iron that results in loss of magnetic properties as the iron passes from the "alpha" to the so-called "beta range." Accompanying the comparatively slight fall of magnetic intensity as the temperature is raised to the Ac2 point, a slight coalescence tendency is manifested. Accompanying the loss of ferro-magnetism occurring at about 785° C. (1445° F.), there is a sudden increase in the coalescence tendency in ingot iron, resulting in noticeable grain growth above this temperature.

Mr. McAdam. It is the belief of the writer, therefore, that the experiments have thrown some light on at least one of the factors affecting grain growth of iron, namely, the effect of temperature on grain coalescence. The conclusions apply undoubtedly to all ingot iron of originally small uniform grain size. Other factors, such as strain gradient, grain-size gradient, and temperature gradient, are of great importance and their effect must be considered in arriving at an understanding of the laws of grain growth.

SOME APPLICATIONS OF MAGNETIC ANALYSIS TO THE STUDY OF STEEL PRODUCTS.

By Charles W. Burrows.

SUMMARY.

This paper is an effort to give the meaning and possibilities of magnetic analysis. Its practical application to the testing of some commercial steel products is discussed. Rails and tools are given special consideration.

SOME APPLICATIONS OF MAGNETIC ANALYSIS TO THE STUDY OF STEEL PRODUCTS.

By Charles W. Burrows.

Introduction.

The possibility of making use of magnetic determinations for estimating the mechanical properties of steel has long been before our minds. At the Budapest Congress of the International Association for Testing Materials in 1901 there was proposed Problem 28, "The consideration of the magnetic and electric properties of materials in connection with their mechanical testing." At the 1912 Congress of the same society the author of the present paper presented a report on this problem. This paper pointed the way to a number of possibilities but was little more than a glance into the future.

Since that time a great deal of experimental work has been carried out, and many questions have been answered. The fundamental principle that there is an exact correspondence between magnetic and mechanical properties has been established beyond doubt. Considerable headway has been made in the practical application of this principle.

The present paper may be considered as a further, though partial, report on the same problem. In it are given very briefly some of the actual accomplishments toward the solution of the problem.

At the present time laboratory tests are being made upon tools, cutlery, springs, ball bearings, cables, rails and a few other articles. Not only are the laboratory methods capable of being expanded into practical shop or commercial tests, but the actual development along certain lines is in progress.

Definition of Magnetic Analysis.

The science of magnetic analysis consists of the systematic correlation of the magnetic and other properties of materials and of the application of the laws and principles which under-lie the interrelations of such properties, particularly the interrelations of the magnetic and mechanical properties of steel. It is based upon the fundamental fact of observation "that there is one, and only one, set of mechanical characteristics corresponding to a given set of magnetic characteristics, and conversely there is one, and only one, set of magnetic characteristics corresponding to a given set of mechanical characteristics."[1]

The art of magnetic analysis consists in the determination of the magnetic characteristics, or better, a small number of the magnetic characteristics, and from these observations making estimations of the mechanical properties.

CRITERIA OF MECHANICAL PROPERTIES.

The nature of a piece of steel may be studied by observing its behavior under the action of certain mechanical forces. The usual mechanical tests result in the destruction of the specimen so that it is not feasible to give such a test to the identical material to be used in a given structure. Even such superficial and intensely localized tests as the Brinell ball hardness test and the scleroscope test require the finishing of the surface tested, and after their application leave the surface in a modified condition.

Another very common way of examining steel is to determine its chemical constituents. The chemical analysis is extremely valuable and is usually considered as being the most valuable criterion of the mechanical possibilities of the steel. This test also must be made on a part of the material which does not enter into the final structure. The assumption must be made that the sample analyzed is substantially of the same composition as material to be used in the construction.

The chemical analysis, however, is entirely inadequate to tell what condition the steel may have been left in as a result of its previous thermal and mechanical treatment. It is well known that two pieces of steel of the same chemical composition may be given different heat treatments and finally have widely different mechanical properties. For instance, slow cooling from a high temperature may leave the steel so tough that it may be bent back on itself without breaking. On the other hand extremely rapid cooling from the same temperature will leave the steel so brittle that it will snap in two when dropped upon the floor.

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[1] Burrows, "Correlation of the Magnetic and Mechanical Properties of Steel," *Bulletin*, Bureau of Standards, Vol. 13, p. 207 (*Scientific Paper No. 272*).

In order to determine whether the steel has had proper heat treatment for a given purpose it is customary to examine the structure of the steel under the microscope. The surface to be examined is carefully polished and etched by some suitable means. The different constituents as determined by the chemistry of the material and modified by its past mechanical and thermal history, are etched to different extents by the etching medium. The resulting etched surface when viewed under reflected light presents an appearance characteristic of the material under investigation. However, the microscopic analysis becomes difficult in the fine-grained steels. Here the structures of steels of slightly different heat treatment are so nearly alike that it is possible to make only a rough estimation of the heat treatment from the microscopic observations.

The mechanical, chemical and microscopic analyses of steel form the basis for the customary specifications and testing. They are, however, open to one or more of the following objections:

1. The test is vicarious. The actual material which is to be used in practice is not itself tested. Usually a certain percentage of the gross amount of material is set aside for test and the remainder is used in construction. This assumes either that the material is all homogeneous or that if non-uniform the test sample is a true representative of the entire lot. It is only by extreme care that either of these assumptions is even approximately met.

2. The test is destructive. In the tension test the specimen is usually broken. The chemical examination changes the nature of the material so that it no longer has the properties of the original. Hardness tests mar the surface to greater or less degree. This destructive nature of a test renders the piece tested unserviceable, no matter how excellent it might have been before the test.

3. The test is local. Many tests do not determine the characteristics of the entire test specimen. A chemical analysis

may be made upon a drilling, which constitutes only one per cent of the sample submitted for test, which sample itself might be considered as representative of one hundred times its own magnitude. A hardness test involves still less material and even this small amount lies within a short distance of the outer surface of the test specimen. The limit of minuteness, however, is attained by the microscope sample. The usual micrograph represents an area of less than one millimeter in diameter and is quite superficial.

4. The test may be qualitative. Some of these tests are valuable only when compared with similar tests on other material which may be considered as reference material. This is true of certain color tests in chemical analysis. The meaning of many micrographs can be determined only by comparing them with photographs of other material of known composition and history.

There are other tests which are free from the above objectionable features. The thermal, electric, and magnetic properties, at least in certain cases, can be measured without altering the nature of the test specimen. If further, there is known the relation which exists between these properties and the corresponding mechanical properties, such tests would be of value in studying the general properties of steels.

The purpose of the present paper is to show how the magnetic examination of steel can be of use to both the producer and user of steel and steel products.

DETERMINING FACTORS IN THE MECHANICAL PROPERTIES OF STEEL.

It is well known that two pieces of steel which differ in any one of a number of particulars show correspondingly different mechanical properties. It is well supported by experimental evidence, but not so generally known, that two pieces of steel which differ in certain particulars, likewise show corresponding magnetic differences. It is not feasible to go into great detail on this point at this time.[2] For the purposes of the present paper it is sufficient to give a few rather well-known examples of this correlation.

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[2] In a scientific paper soon to be published in the Bulletin of the Bureau of Standards will be given many details on the correlation of the magnetic and mechanical properties of steel. This article will contain a rather comprehensive bibliography.

The changes that are brought about by adding certain chemical constituents to steel are well known. In many cases the corresponding magnetic properties have been studied. With increasing carbon content in a hypo-eutectoid steel there is an increase in hardness and tensile strength and a decrease in toughness. Magnetically an increase in carbon content is accompanied by an increase in coercive force and hysteresis, and a decrease in permeability. Many other cases might be cited.

Mechanical operations also bring about corresponding changes in both the mechanical and the magnetic properties of steel. The cold drawing of a carbon steel increases its tensile strength and simultaneously increases its coercive force and hysteresis. Cold drawing also decreases the magnetic permeability

The influence of heat treatment upon the properties of steel is fairly well known. A one-per-cent carbon steel quenched in water from a temperature above its critical point has a greater tensile strength than the same material in the annealed condition. Its hardness is also increased while its toughness is greatly decreased. Magnetically, changes of equal magnitude also occur on quenching. The quenched steel has a greater coercive force, a greater hysteresis, and a lower permeability.

In a specific case a one-per-cent carbon steel in the quenched and the annealed conditions had Brinell hardness numerals, in terms of kilograms per 0.1 mm. indention, of 4390 and 800 respectively. The corresponding values of the coercive forces for a magnetizing force of 150 gausses were 33 and 12.

Steel changes with lapse of time. The course of this change is not so well known as some of the above changes. However, both mechanical and magnetic changes occur. Glass-hard steel softens with age. The same steel shows a decrease in coercive force.

The mechanical properties of a bar of steel may differ from point to point if the bar has not received the same treatment throughout. For instance, if a uniform bar is given a slight bend and again straightened it will show a slight change in its mechanical properties. There will be an increase in both hardness and tensile strength. The same bar will show a corresponding decrease in permeability in the same region.

Even during the operation of stressing a bar we feel sure that changes in the mechanical nature are occurring. Less is known about this phase of the problem than the preceding. However, we do know that small alternating stresses, even though well within the elastic limit, leave the material mechanically fatigued and if the stresses are repeated often enough rupture occurs. While it is difficult to study the changes in mechanical properties after repeated or alternating stresses, the magnetic problem is quite simple and has been investigated to some extent.

PRACTICAL APPLICATIONS OF MAGNETIC ANALYSIS.

Magnetic analysis may be applied to determine the nature of the steel as a raw material, as a finished product, or even after it has been subjected to service conditions.

RAW MATERIAL.

It is obvious that one cannot get a good product from inferior materials. . Consequently it is desirable to know the nature of the raw stock that enters into the construction of the finished product. In this classification of raw material and finished product, it must be recognized that what is raw material from one point of view is the finished product from another. The rod is a finished product as viewed from the standpoint of the rod mill. The manufacturer of ball bearings, however, looks upon the rod as a raw material.

Raw material must be uniform in quality. The proper mechanical and thermal treatment must be determined for each steel that is used. It very frequently happens that two steels of quite different composition may give equally good performance for a particular service provided each steel is given the appropriate heat treatment. In general the heat treatments required for the two steels will be different. Consequently if the heat treatment appropriate to the second steel is given to the first, disastrous results may be expected. It is desirable, therefore, that all the different bars of a given lot of material shall be of the same quality as the material for which the proper heat treatment was determined. Such assurance requires the examination of every individual bar of steel.

The requirement that all bars shall be alike in properties, implies that each bar shall have the proper composition, shall be properly forged, rolled or drawn, as the case may be, and shall be free from local imperfections such as blowholes, segregation, etc.

Occasionally a lot of steel of the wrong composition is supplied through error; or, by some chance, a single bar of different composition may get mixed up with the general stock. If not detected such a bar may cause much loss or annoyance. Very frequently a steel, especially among the alloy steels, may have the proper chemical composition but with the elements not in the proper state of solution. It is not uncommon to find that a tool made from a large bar will prove defective while the same bar when forged down to a smaller size will yield perfect tools. Since the cost of labor frequently amounts to more than the cost of material, it is very desirable to be able to separate the good from the defective material before costly labor has been put upon it.

Another defect that sometimes is found in cold-drawn material is the tearing apart of some of the crystals beneath the outer surface.

Many of these defects would pass unnoticed in the usual examinations by the chemist, microscopist, or mechanical tester. In such cases as the above a magnetic examination may be expected to be of service

FINISHED STEEL PRODUCTS.

The ideal test of a finished product is one which permits the examination of each individual and does not assume that the characteristics of ninety-nine pieces are identical with those of the hundredth one which happened to be picked out for test. Magnetic analysis is adapted to such individual testing. The individual test not only permits the elimination of defective pieces but also permits the grading of a product which, while satisfactory in general, is not all of the same degree of perfection.

It is a matter of common experience that an occasional tool is found which is quite superior to the average fairly good tool. Any method which will enable one to select such exceptionally good tools cannot fail to be helpful. This process of grading will enable the manufacturer to offer a uniformity of product hitherto unknown and permit him to guarantee such uniformity with surety. It is true that certain tools will have to be sold as seconds at a reduced price. On the other hand there will be a new class of exceptional quality which will bring a correspondingly higher price.

The individual test will permit improvement in design. At present we insist upon factors of safety so large that there would be a reasonable factor of safety even though there were a considerable amount of defective material. If a material is used whose individual properties are not known and which may vary between wide limits a greater factor of safety is used than if known material were used. For example, a cast-iron structure might have a factor of safety of ten while a similar structure of steel would be safe with a factor of four or five.

Frequently it is desirable to make exhaustive tests upon a few pieces. Such cases arise where one or more samples are submitted in competition with other material. I recently witnessed a test on the results of which an order of $200,000 was to be based. Five pieces worth about $2.00 each were submitted by each competitor. The properties of each sample depended upon proper composition and proper heat treatment. Even though the composition were correct a slight error in the heat treatment would place the material at a disadvantage. By a suitable magnetic test the manufacturer could have assured himself that his samples were representative of his best product.

I have made laboratory tests on a number of commercial articles with considerable success. Experimental evidence already at hand shows that it is feasible to apply individual magnetic tests to many forms of tools, cutlery and springs. These tests are of such a nature that they may be carried out on a commercial scale. Rings and balls for ball bearings are now being tried out by this method with every promise of success. Steel rails, in spite of their size, lend themselves very readily to this method of examination.

SERVICE EFFECTS.

Since the magnetic and mechanical properties of steel suffer changes with the lapse of time and under ordinary service conditions, the magnetic method offers a very promising means of examining, without destruction, the changes that develop during the life history of a structure. Some of the things to be looked for are the ordinary results of wear and tear. A good example of this is the mine or elevator cable. Such cables grow old and lose their usefulness or even become a source of danger due to the wearing away of the material, the breaking of strands, excessive strains, the development of a crystallin structure, etc. The causes of deterioration of a cable are so numerous and the possibility of disaster frequently so great that it is customary to replace an old cable by a new one while several years of usefulness remain in the cable. A proper magnetic test would keep the cable under surveillance at all times and make an autographic record.

A structure may fail by the development of flaws. For instance, a steel rail may render apparently satisfactory service for several years and finally develop a transverse fissure which results in a dangerous accident. Periodic examinations of rails which are under suspicion because of excessive duty or other causes may be made and we may thus detect the presence of such a flaw in an early stage of its development.

Magnetic analysis may be of use in the development of a new design. It is customary when a new design or a new model is developed to run a service test and at intervals to dismantle the apparatus and go over in detail the various members of the structure. This procedure is common in high-grade mechanisms such as automobiles, adding machines, etc. Such an examination will detect errors in design or faulty material which result in fracture or excessive wear. They do not, however, indicate the structure of a part which is just about to break. A magnetic test may be expected to follow the changes in structure from the initial perfect condition, through the various stages of fatigue, to the final rupture. Such changes would be indicated magnetically, whether they were due to a gradual return to a crystallin equilibrium or to stress effects.

Another field which has not as yet been tried but for which the possibility has already been established is in the study of the state of stress of a given structural member. For instance, it may be desired to determine whether a given member in a bridge undergoes tension or compression during the passage of a train. The fact that steel suffers certain changes in its magnetic properties when put under tension and certain other changes when put under compression renders such a determination possible.

ROUTINE TESTS.

The apparatus required and the procedure of testing depends to a great extent upon the nature and size of the material tested as well as upon the characteristic flaws to be expected. Apparatus for examining safety-razor blades is necessarily quite different, notably in size, from apparatus for the testing of steel rails. In the case of a lathe tool in which only the nose is hardened the examination differs from that of a locomotive driving rod which is more nearly uniform throughout.

RAILS.

If the testing of rails is to give a certainty of the quality of the material, it is necessary to test not only each rail but every element of the length of each rail. This may be accomplished by a double magnetic test. One part of the magnetic test will determine whether there are non-uniformities along the length of the rail. Since this test is based upon a determination of magnetic leakage it is referred to as the "leakage" test. The leakage test determines the homogeneity of the rail. Inhomogeneities, while they may not be due to dangerous imperfections, always cast suspicion upon the rail. Inhomogeneous rails should not be used in places where the highest degree of perfection is required. For instance, inhomogeneous rails should not be used on bridges, in tunnels, or where the traffic is heavy. Homogeneity in itself is not evidence of perfection. A rail may be quite uniform along its length and yet be an unsafe rail. Its dangerous nature may be due to improper chemical composition or to errors in the process of manufacture

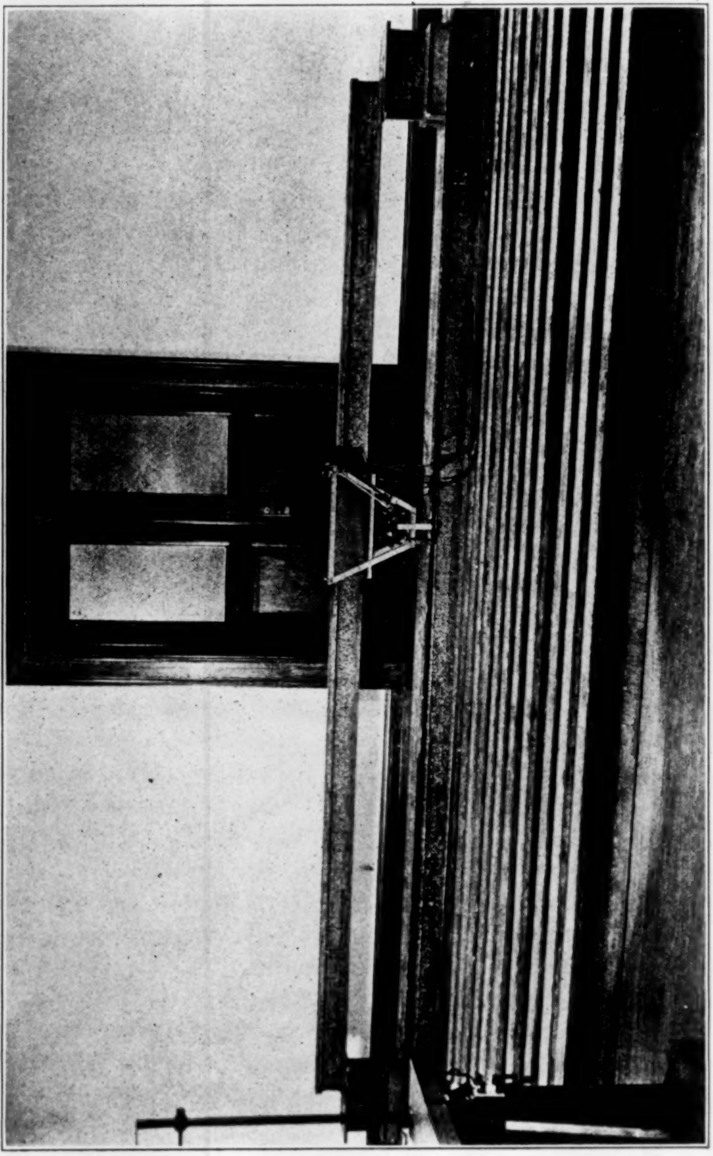

FIG. 1.—The Magnetic Circuit of the Rail under Examination by the Leakage Test.

such as improper finishing temperatures or excessive cooling rates. To determine whether or not such defects exist requires a determination of one or more magnetic constants. These measurements may be made in some form of permeameter and in conformity with the nomenclature for similar tests on small specimens, we call this part of the magnetic examination the "induction" test.

Figs. 1 and 2 show the general appearance of the leakage apparatus for rails. A magnetizing solenoid is moved along the length of the rail. Any non-uniformity along the rail will cause a variation in magnetic leakage. This variation in leakage develops a small electromotive force which is approximately proportional to the degree and sharpness of the non-uniformity. A special recording voltmeter, not shown on the photograph, makes a photographic record of the magnetic inhomogeneities. This examination of a rail can be made in about one minute and excessive demands for skill and training are not made upon the operator.

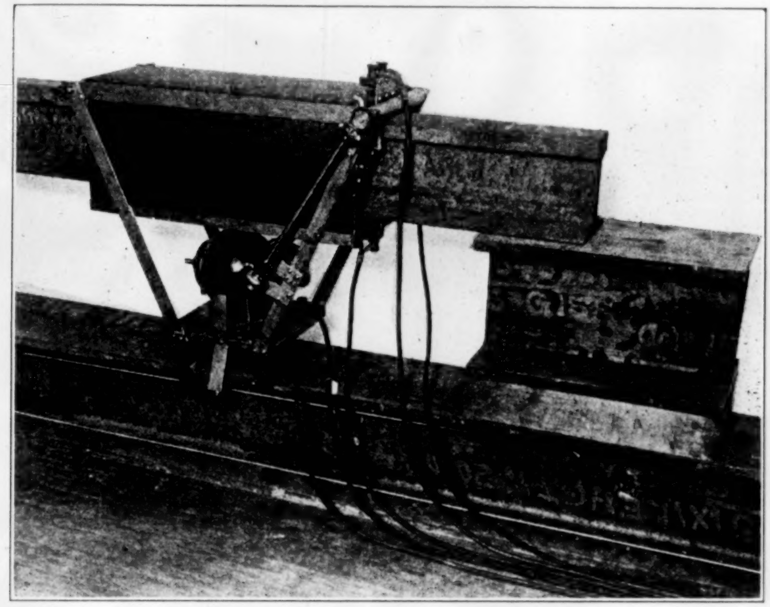

FIG. 2.—The Traveling Solenoid Used in the Leakage Test of Rails.

Fig. 3 shows a set of magnetic-leakage records of a 105-lb. rail before and after gagging. This rail has the following chemical composition:

Carbon	0.68	per cent
Manganese	0.98	"
Phosphorus	0.025	"
Sulfur	0.031	"
Silicon	0.144	"

It is one of a set of rails which was expressly prepared for this investigation. The rails as they left the hot bed were practically straight and therefore did not need to go through the gag press.

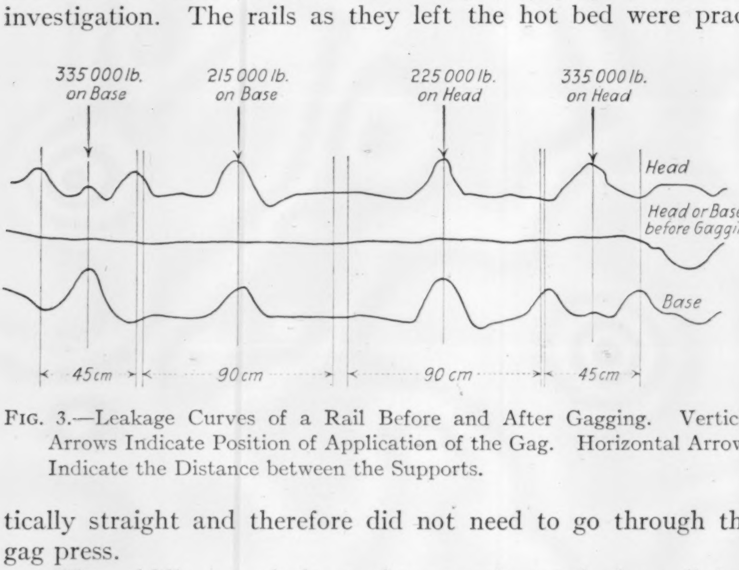

Fig. 3.—Leakage Curves of a Rail Before and After Gagging. Vertical Arrows Indicate Position of Application of the Gag. Horizontal Arrows Indicate the Distance between the Supports.

The middle record shows the normal record of a rail free from inhomogeneities. The irregularity in the curve at the right-hand end is due to the starting of the apparatus and therefore has no significance. From these curves several observations may be made.

1. The rail before gagging shows a magnetic-leakage curve which is substantially a straight line.

2. Pressure applied locally so as to deform the metal produces a magnetic inhomogeneity at the point where the pressure is applied. This is true whether the pressure is applied on the head or on the base.

3. The extent of this local inhomogeneity depends upon the distance between supports. If the supports are far apart the strain seems to extend over the entire cross-section and seems to be greater on the side of the rail opposite the point of application. If the supports are close together the strains are localized on the side on which the pressure is applied.

4. The strains set up by the supports depend upon the distance apart of the supports. If the supports are 90 cm. apart the magnetic effect of the supports is insignificant. If they are only 45 cm. apart this effect is considerable. In fact, the side of the rail on which the supports lie shows a magnetic inhomogeneity at each support which is greater than the corresponding inhomogeneity opposite the gag.

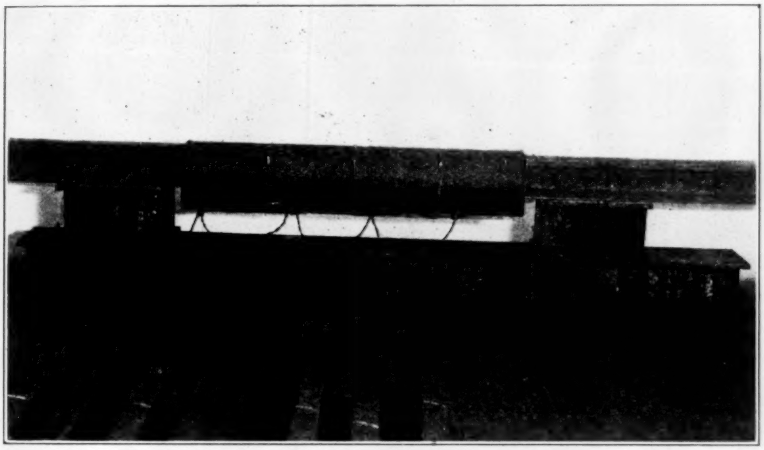

Fig. 4.—A Simple Type of Rail Permeameter.

5. Other observations on other rails show that the degree of magnetic inhomogeneity increases with the load applied by the gag

6. Furthermore it is worthy of note that another rail from the same ingot which had been annealed showed a much less effect for the same gagging stress.

This ability of the magnetic leakage test to indicate the effect of gagging is of importance in view of the fact that many people consider that excessive gagging produces minute failures in the rail which are in effect potential transverse fissures.

The induction test is fairly simple. Fig. 4 shows the test rail and magnetizing solenoids in position. The accessory apparatus which is similar to that required in induction measurements of small rods, is not shown. The most useful magnetic quantity is probably the coercive force. For the purpose of illustration we may give the coercive forces of two lots of rails of substantially the same chemical composition, made at the same mill, and differing only in their method of cooling. Four rails rolled in winter were from the first ingot which passed through the mill in starting the round for that particular date. Consequently these rails were the first upon the hot bed which was initially cold. Two other rails of substantially the same chemical composition were selected from a full hot bed and from the middle of a round rolled when the atmospheric temperature was 70° F. The averages of the coercive forces for a magnetizing force of 145 gausses were 10.30 gausses for the winter rails and 8.91 gausses for the summer rails. This difference of 1.39 is probably due in great part to the difference in the rate of cooling of the two sets of rails.

TOOLS.

Fig. 5 shows a type of apparatus designed especially for the testing of drills, lathe tools, cutlery, etc. It is so arranged that the portion tested may consist of the major portion of the piece or it may be limited to the end portion.

A number of other pieces of apparatus suitable for magnetic analysis have been described more fully elsewhere.[1]

COMMERCIAL MAGNETIC ANALYSIS.

The fundamental fact that there is a definite relation between the magnetic and mechanical properties of steel is so well established that the successful application of magnetic

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[1] Burrows, "The Determination of the Magnetic Induction in Straight Bars." *Bulletin*, Bureau of Standards, Vol. 6, p. 31 (*Scientific Paper No. 117*).

Fahy, "A Permeameter for Magnetic-Mechanical Testing." *Electrical World*, Vol. 69, pp. 315–317 (1917).

Burrows and Fahy, "Magnetic Analysis as a Criterion of Steel and Steel Products." *Bulletin*, Bureau of Standards (a forthcoming Scientific Paper).

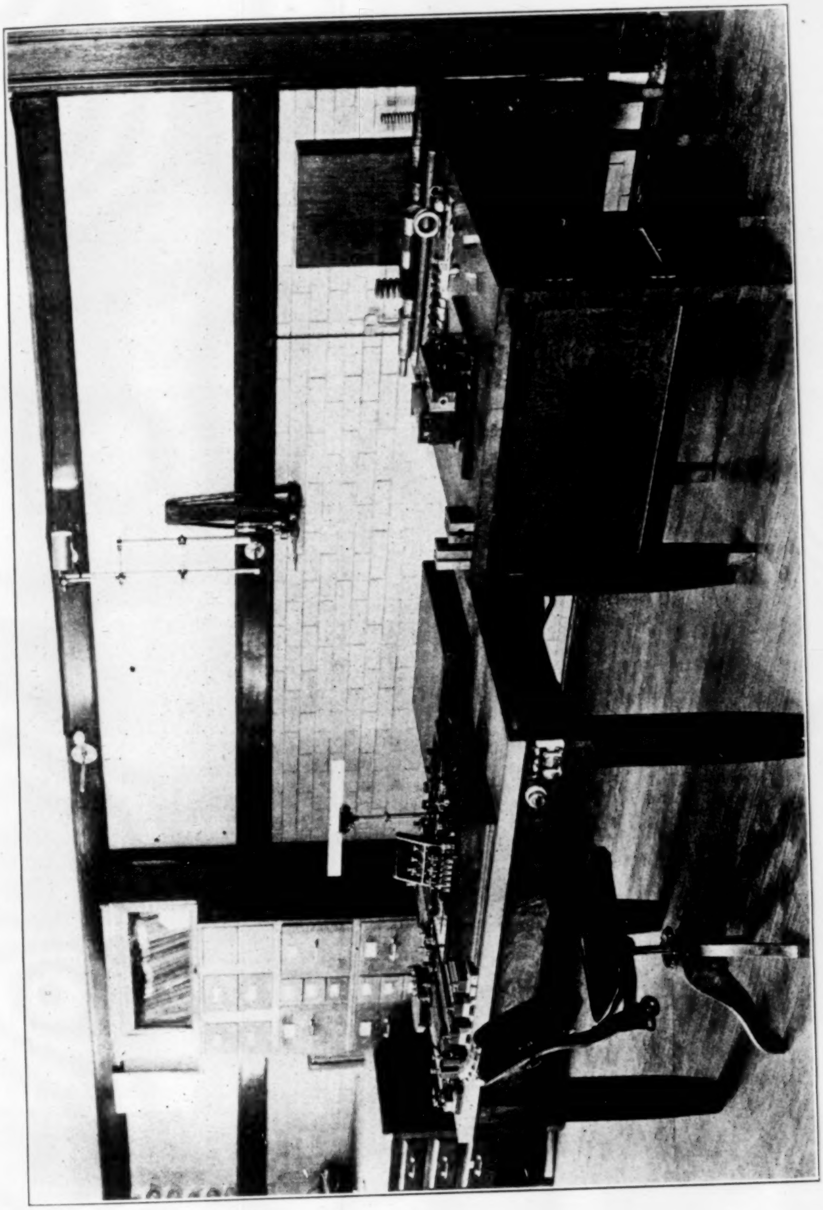


FIG. 5.—The Complete Magnetic Apparatus for Testing Tools, Cutlery, etc.

analysis to commercial testing is assured. In any particular case the apparatus must be developed and operators trained. However, the difficulties presented are not excessive. Before shop methods of examination of any given product can be installed, a preliminary investigation must be carried out which involves five steps.

1. Magnetic data of representative material must be determined. Good, bad and indifferent materials must be examined and the data thus obtained correlated so that one may know what magnetic characteristics go with each grade of material.

2. From the mass of data thus obtained the most suitable magnetic criteria must be determined. In some cases any one of several magnetic constants may be equally satisfactory. In other cases only one specific constant may be used. Still others may require the combination of two or more magnetic constants.

3. Suitable apparatus must be designed. After the proper criteria have been determined the next step is to determine what type of apparatus is best suited for the particular material to be tested. Ball-bearing rings will, for the most efficient operation, require a slightly different piece of apparatus from that required by twist drills.

4. Limits of quality must be determined. Since the proper combination of speed, cost and accuracy may call for some slight deviation from the ideal theoretical considerations, the material to be tested must be examined by the final form of apparatus. From these data the final criteria of quality must be determined.

5. The final step of this preliminary investigation is the reduction of all the operations to mere routine, so that shop determinations may be made by an observer who is not necessarily an investigator.

It is not feasible to make an estimate of the cost of the testing equipment. However, it would not be excessive. The rail-testing apparatus shown could be duplicated for about \$1500. While this apparatus would undoubtedly require modification for shop practice, this amount may be considered as a rough estimate of the cost.

DISCUSSION.

Mr. Unger. MR. J. S. UNGER (*presented in written form and read by the author*).—The paper by Mr. Burrows must be studied in conjunction with Bureau of Standards Scientific Paper No. 272 by the same author, to obtain an idea of the work done so far. The present paper refers more particularly to rails and tools, and it is from the rail-testing angle that I present this discussion.

Magnetic testing does not destroy or deface the material and to a certain extent it permits of testing every piece if of some length, such as a rail or drill. It is admitted, however, that short heavy objects, such as castings, are difficult if not impossible to test. The apparent advantage of testing every rail is not new, as every rail is and always has been tested automatically during the process of straightening. It has been shown that magnetic and mechanical properties bear a relation to one another. This same statement may be made of the chemical composition or heat treatment when compared with the mechanical properties.

Magnetic testing will show a pipe if of considerable extent. As there are almost diametrically opposite opinions among railroad authorities as to whether a pipe is injurious in a rail, must this not be settled first?

The effects of cold rolling or drawing, torn surfaces or interior defects produced by cold work may be shown. A rail in service is cold-rolled on the head, which is an essential part of the service it performs. A locomotive standing on a track will stress the rail under each wheel. Such stresses can be magnetically shown. If the locomotive be moved or the tie supports changed, the stresses change to new points. Magnetic disturbances are shown at each tie in Scientific Paper No. 272.

It is claimed a rail may be magnetically homogeneous but unfit for service, due to improper chemical composition, finishing temperature or excessive cooling rates. Rails are not made of either very soft or very hard steels but within reasonably close chemical requirements, yet some rails do fail. If rail finishing

Mr. Unger. temperatures considerably above and below the limits prevailing at different mills affect the quality, why does the actual experimental evidence from the track not prove it? Winter and summer rails are shown with different magnetic properties attributed to different rates of cooling. The inference is, that the more rapidly cooled rails are inferior. If this be true, why are some railroads considering plant installations to heat and artificially cool rails more rapidly, and how can we explain a difference of 50 per cent less wear in oil-quenched rails after five years' service when compared with a similar number of untreated rails under exactly the same conditions of service, no failures occurring in either.

If a gagged rail shows a magnetic disturbance where gagged and an annealed rail a lesser effect, should rails not be ordered annealed or what accomplishes the same purpose, of a softer variety chemically? If such be done will the rails not require more frequent renewals on account of wear? Referring again to the gagging, no truly comparative track evidence has been presented, so far as I know, to prove that it is injurious. I have not been able to prove it from gagged and ungagged rail experiments made in the track.

It is recommended that magnetically inhomogeneous rails should not be used on bridges. The data on rail failures do not indicate that rails fail at this point, probably due to their better support. If rails are inhomogeneous, would it not be better to discard all such rails, or if they must be used, to install them at such points where failures rarely occur?

Reference is made to the detection of flaws such as transverse fissures and to the periodic examination of rails in service. I am interested in knowing if a fissure has been detected in an apparently sound rail and if it can be detected after such a rail is laid, or does it only appear after being in service for some time.

Within three years at least five methods have been suggested for testing rails. The question arises, have these tests where used positively shown the fitness or unfitness of a rail for actual service? Is there not a greater need for the correlation of the present tests with the data from actual service, than for any new method of testing? I believe a study of the conditions existing in the track is of equal importance with a study of the rail. **Mr. Unger.**

Mr. Howe. MR. HENRY M. HOWE.—I think this paper of Mr. Burrows must be looked on as a step forward in a very important direction. It seems to me that Mr. Burrows is one crying in the wilderness. We are the wilderness. Our tests destroy the part tested. That is a crude state of the art of testing. We are in the beginning of the art of testing. The world will have a long time to live after we get through with it. A wholly different line of testing from that which we now employ is opened up by Mr. Burrows' investigations; that is to say, the reaction of the material to forms of energy which have no permanent effect on the material itself. He determines the reaction of a substance to magnetic tests. That is only one of many ways, many forms of energy, the reaction to which can be determined. We must not expect that this method, in its infancy, is going to leap into immediate and very extensive usefulness; but here is a beginning. It is a beginning in my opinion in the most important direction which we can possibly take, and I disagree with reluctance from my greatly esteemed friend, Mr. Unger, for I do believe that we need radically different methods of testing, that our present methods are those of a crude ignorant age, and will give way in time—they may not in my time—to radically different methods of testing which determine the reaction of the substance to forms of energy which do not injure the substance.

Mr. MacPherran. MR. R. S. MACPHERRAN.—I should like to ask Mr. Burrows what qualities show for superiority in testing. What are the qualities found in the best grade of samples?

Mr. Voorhees. MR. S. S. VOORHEES.—I should like to ask Mr. Burrows if he is able to measure by this means the strains which have developed in the material after use. If that is possible, it will introduce a very valuable line of investigation.

Mr. Burrows. MR. CHARLES W. BURROWS.—If a piece of steel is magnetized the quantity of magnetization which it takes up is called the magnetic induction or simply the induction. The magnetizing force is the quantity which causes the induction.

The magnetic induction increases with increasing values of the magnetizing force. The corresponding curve, however, is not a straight line. With increasing magnetizing forces the

Mr. Burrows. induction increases at first slowly, then rapidly and finally more slowly. The curve thus shows three distinct stages. If the magnetizing force, after it has been increased to a certain value, is gradually decreased, the induction will decrease but will not follow the same course as under an increasing magnetizing force.

When the magnetizing force has been reduced to zero the specimen still retains some magnetization; this is called the residual induction and is represented by the symbol B_r. To remove this residual induction it is necessary to apply magnetizing force in the reversed direction, beginning with a small value and continuously increasing it until the induction is entirely removed from the specimen. This reversed magnetizing force necessary to reduce the induction to zero is called the coercive force, and is represented by the symbol H_c.

The coercive force is one of the most important of the magnetic characteristics for the estimation of mechanical properties. The permeability, which is the ratio of the induction to the corresponding magnetizing force, is also of use; likewise the residual induction. The coercive force, however, is the more important. For instance, in the heat treatment of a given piece of steel, the annealed condition may have a coercive force of 10 gausses, while in the quenched condition the coercive force is 30 or 40 gausses. Intermediate conditions of hardness due to drawing of the quenched material will give intermediate values of the coercive force. Consequently the coercive force is to some extent, at least, an index of the heat treatment.

Work has been done in my laboratory and elsewhere which shows that the coercive force will indicate differences in the quenching temperatures as small as it is feasible to measure with the pyrometer. We have been making magnetic tests on specimens whose quenching temperatures differed by 15 to 25° C. The corresponding differences in coercive force are quite large and easily measurable.

Steel rails may be examined by the same method. However, we have been looking for the existence of inhomogenities along the length of the rail, due to defects of various kinds. For this purpose we use the permeability rather than the coercive force as a criterion. The procedure is to subject each cross-section successively to the same magnetizing force and measure the varia-

Mr. Burrows.

tions in permeability. The record may be photographic and a rail may be thus examined over its entire length in less than one minute.

With reference to the question as to whether long specimens are the only ones that are suitable for magnetic measurement, it is true that the long specimens are the most suitable shape for measurement, and some time ago I was prepared to acquiesce in the idea that one must have a relatively long specimen if satisfactory results are to be expected. Lately, however, I have been working with specimens which are shorter in length than they are in diameter. I refer to the rings which are used in ball-bearing races. Differences in the coercive force due to differences in the heat treatments of such specimens show up very markedly. The same is true for knife blades, and for drills ranging from $\frac{1}{8}$ to 1 in. in diameter. I have also gotten some satisfactory results from the round balls used in ball bearings.

Mr. Voorhees has asked whether this method is applicable to the study of a strained condition in the metal. Magnetic analysis may do much in the investigation of such strains and in the study of mechanical fatigue. Steel under a moderate tension shows a higher permeability than the same steel free from stress. Steel under compression shows a decreased permeability. By means of suitable apparatus it would be possible to tell whether the application of the load, for instance a train entering upon a bridge, causes a given member to undergo tensile or compressive stresses.

I have just recently built an electromagnetic tuning fork for the study of repeated stress effects. The procedure is to subject the prongs of the tuning fork to alternating stresses and at intervals measure the magnetic properties. It is hoped that the gradual fatigue and break down of the fibers will be indicated by corresponding magnetic changes.

If this experiment turns out successfully its applications are unlimited. Suppose, for instance, that a new model of an automobile or any other machine were subjected to a service test continued to destruction. Magnetic tests at different time intervals would indicate structural changes in the steel, long before actual failure occurs. The information thus obtained would indicate where improvements in material or design were needed.

Mr. Gibbs. MR. A. W. GIBBS.—I should like to ask Mr. Burrows whether that method gives any chance of examining the material *in situ*. Speaking of rails, it has been found that frequently several concealed fissures run in the heat. Is it possible to examine them? At present the only thing to do, if you have several in one heat, is to take the heat and break it up. That does not do much good except that the heat is dead and can do no more harm. Does this method lead to any probability that this magnetic apparatus can ever be passed over the rail in position and pick out the rails with concealed defects, and thus insure that only sound rails will remain in service?

Mr. Devries. MR. R. P. DEVRIES.—The magnetic test described by Mr. Burrows has been a subject of interest at the Bureau of Standards for some four or five years. Although I have left the Bureau of Standards, I have not left my interest in magnetic testing behind there.

The magnetic test may in a good many instances serve the purpose of identifying the material which is being tested. When our tests do not differentiate between steels which we know are different because they have been treated differently and have given widely different service records, then it certainly becomes important to apply a test which will perform this differentiation.

Briefly, I wish to call attention to three specimens of steel, one annealed and two in a hardened condition. These pieces were tested by means of the Brinell hardness test, notch bar impact and the Martens scratch test. These tests easily differentiated between the annealed and the hardened specimens, but did not distinguish between the two hardened specimens. In Fig. 1 the "*B-H*" curves for these specimens are placed. They indicate that by differentiating between the annealed and hardened state the test also discloses considerable differences between the two hardened specimens which the mechanical tests do not show. This is the point that I should like to emphasize: the magnetic test, even if it does nothing more than occasionally identify material, will probably serve a very useful purpose indeed.

Mr. Fahy. MR. FRANK P. FAHY.—In discussing this question of a magnetic test, several other points are worthy of consideration at this time. It is important to bear in mind that the magnetic

Mr. Fahy.

data enable one to form conclusions as to the uniformity of the material over its section. That is, the test results do not depend solely upon the properties of the exterior surface or the portions lying contiguous thereto. Moreover, in material of irregular section, as for instance a rail, it is possible to isolate to a considerable extent, for purposes of test, the head, web and base and to obtain indications of their respective magnetic characteristics.

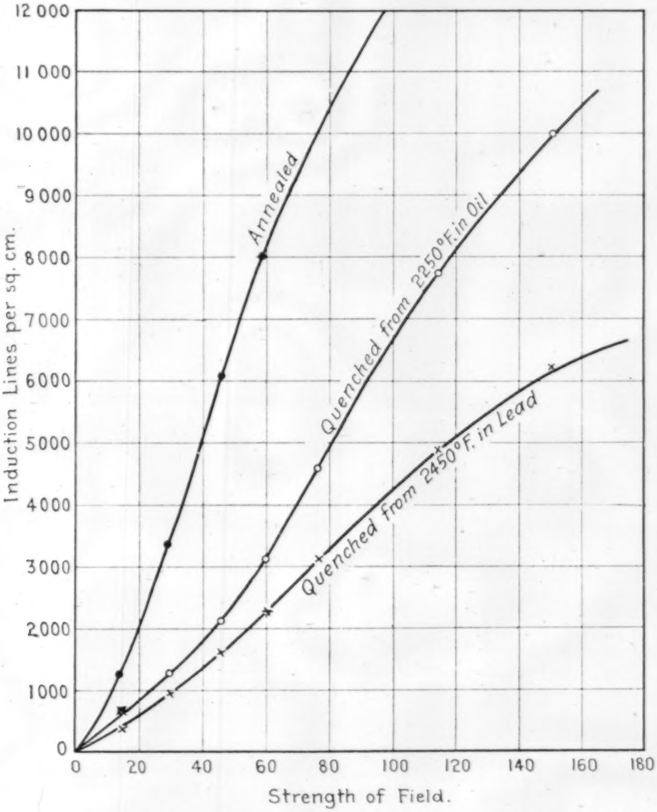


FIG. 1—"*B-H*" Curves of Tungsten Steel.

The magnetic phenomena which are observed when a magnetic force is applied to a steel product are strongly influenced by the structural characteristics obtaining. It is sometimes found

Mr. Fahy. that a steel, and particularly some of the alloy steels, which by the usual hardness tests are apparently in good condition, are so brittle that they readily snap under stress. In such cases the important magnetic quantity, the coercive force, is observed to be abnormally high.

Some magnetic investigations conducted abroad within the past three years on steel which was greatly oxidized, have shown that this condition is indicated to such an extent as to bring about an increase in the coercive force amounting to 250 per cent. Steel cold worked or strained shows considerable change in its magnetic properties, and this method of test has been used to indicate the degree of mechanical working to which material has been subjected.

We have found in our practice that it is frequently possible to tell, from the magnetic characteristics of a finished heat-treated product, where a quenching operation has been followed by one of reheating, whether the initial operation of quenching was correctly performed. In other words, drawing a quenched object, even where the drawing temperature is quite high, usually does not conceal the magnetic characteristics which are typical of inefficient quenching. In one instance a variation in temperature of 70° F. which was followed by a drawing temperature of 900° F. was strikingly observable, although the quenching temperatures compared were both well above the critical range.

Mr. Burrows. MR. BURROWS.—The examination of rails in the track, while not impossible, is one of the more difficult phases of the problem. The entire problem of magnetic analysis is a new line of work. I have been engaged in this personally, possibly eight years or so, and have accomplished some things, though much remains to be done. It is not necessary that the rail to be examined be encircled by any apparatus; there is no theoretical reason why it cannot be examined in the track. I am planning at the present time to examine elevator and mine cables in service by means of adjacent but not encircling apparatus, and to secure a continuous autographic record similar to that of a recording electrical instrument.

Mr. Devries brought out the point that it is possible to differentiate materials. I might give just one instance. Some time ago, perhaps six or seven years ago, I had my mechanician

Mr. Burrows.

prepare a $\frac{1}{2}$-in. rod about 10 in. long, cut in the middle, and then put the two halves together to the very best of his ability, removing as little of the metal as possible. After it was all finished, it was ground and polished so that it was impossible to detect visually where the joint was. I examined this bar magnetically by measuring the variations in the permeability along its length. There was a marked magnetic irregularity where the two parts of the bar had been joined. Several years later, I wished to use this bar again and had to pick it out from amongst about fifty bars of the same length and diameter and all somewhat rusty. I examined a number of bars and tried to unscrew them, but was unable to locate the bar with the joint. Finally I examined them magnetically, and all the uniform bars had the same magnetic uniformity. When I came to the one that had been cut, I got the same magnetic peculiarity which I observed in the first place.

I might say that what I have presented in this paper is merely a note intended solely to arouse interest, and I think that purpose has been accomplished to some extent. There is a more extensive publication on this subject in progress at the present time, and I would be very glad to see that any of you who may be interested in it shall receive it. This work is being carried on at the Bureau of Standards and you will be welcomed, any or all of you, to talk over any particular problem in which you may be interested. I am interested in seeing the science of Magnetic Analysis applied to as many practical problems as possible. Heretofore we have spent our efforts on the theoretical aspect and the main conclusion we have reached is that it is possible to apply such a magnetic test.

HARDNESS OF HARD-DRAWN COPPER.

By E. H. Peirce.

SUMMARY.

Tests have been made to determine whether hard-drawn copper wire possesses a greater hardness and tensile strength in the section near the surface than in the remainder of the section.

Various samples of hard-drawn copper wire were tested for Brinell hardness, both on the surface and at various points in the cross-section.

Various samples of hard-drawn copper wire were tested for tensile strength. Tensile tests were also made on similar samples from which the original surface had been removed by (1) turning in a lathe and (2) dissolving in nitric acid.

Some phases of the mechanism of cold drawing are considered in the discussion of the data obtained from the tests.

The conclusion is that hard-drawn copper wire is equally affected throughout its mass by the cold-drawing process, and hence, that the common conception of a hard, exterior "skin" is erroneous.

HARDNESS OF HARD-DRAWN COPPER.

By E. H. Peirce.

The object of the investigation to be discussed in this paper is to determine whether hard-drawn copper wire is harder near the surface than in the interior. There exists a rather common belief that hard-drawn copper wire possesses a "skin" or surface layer of considerable thickness, which is of greater hardness and tensile strength than the remainder of the section, and that beneath this "skin" the copper has been practically unaffected by the drawing process. The belief that such a condition exists has even influenced engineers in their selection of material for certain purposes.

In the light of Beilby's amorphous theory, we can readily conceive of an immeasurably thin layer of amorphous metal formed on the surface of hard-drawn copper wire, due to the surface friction between the drawing die and the wire. The term "skin" as popularly used in connection with hard-drawn copper wire, refers to a supposed layer having a thickness of several hundredths of an inch, and not to the immeasurably thin layer of amorphous metal referred to above.

In order to secure definite data on this matter of fundamental importance, numerous samples of hard-drawn copper wire have been subjected to various tests in order to furnish evidence as to the nature of the hardness produced by the drawing operation. Tests have been conducted on samples of commercial copper wire in various sizes which are in common use. Even the largest sizes are too small, however, for satisfactory investigation with the regular Brinell hardness tester using a 10-mm. ball. Hardness tests have therefore been conducted with a modification of this test. The ball used was 0.1875 in. in diameter and was under a standard pressure of 120 lb. The impressions were read with a microscope at 50 or 100-diameter magnifications. The test was standardized against the regular Brinell test, using for the standardization, pieces of metal of uniform hardness and of such size that the standardizing specimen could be measured for hardness by both the large machine and the small modified machine. Numerous check tests have demonstrated the reliability of this modified test, which has the marked advantage of making possible Brinell hardness tests on very small sections, and of making possible numerous hardness tests at points very near to each other in sections too small for even one satisfactory hardness test with the 10-mm. ball. This test measures the hardness of very thin sections. In material of a hardness number of 100 the impression has a depth of 0.0016 in.

Two kinds of tests have been used to secure information as to the nature of the hardness of hard-drawn copper, namely, Brinell hardness tests and tension tests.

BRINELL HARDNESS TESTS.

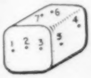
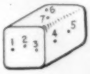
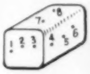
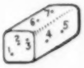
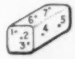
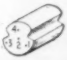
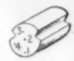
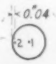
In order to secure a flat hard-drawn surface suitable for Brinell hardness tests, without preparatory treatment of any kind, several samples of hard-drawn copper wire were produced having a square cross-section. The cross-sections were also ground and polished for hardness tests. Tests were made on the unprepared surface of the wire, and again with the surface prepared by giving it the least polishing required to remove drawing marks. The unprepared surfaces were tested to eliminate any question as to the effect of preparation of the surface on the hardness test. The prepared surfaces were tested so that these results would be comparable with the tests of the cross-sections, which necessarily have prepared surfaces.

The results of the hardness tests are given in Table I.

TENSION TESTS.

Samples of common sizes of hard-drawn round copper wire were subjected to tension tests. From like samples the cylindrical surfaces were removed to various depths by turning in a lathe and the reduced cross-sections tested for tensile strength. From other like samples, the cylindrical surfaces were removed to various depths by chemical means, that is, by treating with nitric acid until the diameter was reduced by the desired amount. One part of nitric acid (sp. gr. 1.42) to one part water was used.

TABLE I.—RESULTS OF BRINELL HARDNESS TESTS.

Specimen No.	Description.	Tensile Strength, lb. per sq. in.	Kind of Test Specimen.	Location No.	Brinell Hardness Number.	Preparation of Surface.
1	Square wire drawn 1 draft from 0.620-in. round rod to 0.497 by 0.500 in. in cross-section.	45 000		1	100	Polished.
				2	102	"
				3	100	"
				4	102	"
				5	104	"
				6	100	As drawn.
				7	102	"
2	Square wire drawn 1 draft further than in specimen No. 1 to 0.434 by 0.434 in. in cross-section.	51 100		1	117	Polished.
				2	122	"
				3	122	"
				4	108	"
				5	108	"
				6	104	As drawn.
				7	104	"
3	Square wire drawn 1 draft further than in Specimen No. 2 to 0.409 by 0.409 in. in cross-section.	53 200		1	113	Polished.
				2	122	"
				3	113	"
				4	104	"
				5	104	"
				6	108	"
				7	100	As drawn.
				8	106	"
4	Square wire drawn 1 draft further than in Specimen No. 3 to 0.326 by 0.328 in. in cross-section.	60 000		1	131	Polished.
				2	126	"
				3	126	"
				4	122	"
				5	122	"
				6	117	As drawn.
				7	117	"
5	Square wire drawn 1 draft further than in Specimen No. 4 to 0.301 by 0.299 in. in cross-section.	62 000		1	135	Polished.
				2	135	"
				3	122	"
				4	122	"
				5	117	"
				6	122	As drawn.
				7	117	"
6	4/0 cold-drawn grooved trolley wire 0.480 in. diam. (2 drafts from $\frac{19}{32}$-in. rod).	51 800		1	122	Polished.
				2	128	"
				3	117	"
				4	122	"
7	4/0 cold-drawn grooved trolley wire 0.480 in. diam. (2 drafts from $\frac{19}{32}$-in. rod).	51 800		1	113	Polished.
				2	122	"
				3	122	"
				4	108	"
8	0.528-in. annealed wire..........	.....		Center	48	Polished.
9	2/0 trolley wire drawn from the 0.528 in. annealed wire of Specimen No. 8.	.....		1	113	Polished.
				2	109	"

Reduced cross-sections were tested for tensile strength. The results of tests on the various specimens are given in Table II.

DISCUSSION OF RESULTS.

Attention is drawn to the large increase in hardness due to cold drawing; the hardness of a rod or annealed wire (see Table I, sample No. 8) being less than one-half of that of the hard-drawn wires. Therefore, if the drafting process were to produce a hard skin on the surface, leaving the remainder of the cross-section practically unaffected, we should expect to secure a Brinell hardness number on the centers of the hard-drawn wires in the neighborhood of 50. All of the figures obtained, however, show 100 or higher.

The results of Table I show that the surface of hard-drawn copper is not higher in Brinell hardness than the inner portions. The plastic deformation produced by cold drafting has evidently hardened the wire throughout its section. This is true of the first drafts as well as of subsequent drafts.

The results of Table II indicate that the removal of the surface metal of hard-drawn wire has no appreciable effect on the unit tensile strength of the remaining metal. Surfaces were removed both mechanically and chemically and the two methods are apparently equivalent as regards their effect on the unit tensile strength of the remaining metal.

About 0.092 in. thickness of metal was removed by each of these methods from lots Nos. 1, 2, 3, 4, 5, 6, 8, 9, 10 and 11, making a total of 20 specimens on which the results could be compared. Of these 20 samples, 5 samples showed that the unit tensile strength of the remaining cross-section was less than the unit tensile strength of the original wire, while with the other 15 samples the unit tensile strength of the remaining section was somewhat greater than that of the original wire.

On lots Nos. 7, 12 and 13, which were not included in the comparison above, the surface was removed to a less depth. The results on these lots are, however, similar to those discussed above.

Evidently, the effect of the plastic deformation produced by the cold drafting has been to increase the unit tensile strength of the metal uniformly throughout the section. This is true of

TABLE II.—RESULTS OF TESTS FOR TENSILE STRENGTH.

Description of Specimen.	Layers Removed from Surface Mechanically.			Layers Removed from Surface Chemically.		
	Diameter, in.	Thickness of Layer Removed, in.	Tensile Strength of Remaining Cross-Section, lb. per sq. in.	Diameter, in.	Thickness of Layer Removed, in.	Tensile Strength of Remaining Cross-Section, lb. per sq. in.
4/0 wire, lot No. 1......	0.4560	0.0	51 300	0.4560	0.0	51 300
	0.3916	0.0322	50 300	0.3920	0.0320	51 200
	0.3815	0.0373	50 100	0.3814	0.0373	50 200
	0.2711	0.0925	51 500	0.2711	0.0925	52 000
4/0 wire, lot No. 2......	0.4560	0.0	51 000	0.4560	0.0	51 000
	0.3913	0.0323	50 900	0.3922	0.0319	51 000
	0.3810	0.0375	51 300	0.3828	0.0366	51 300
	0.2728	0.0916	52 300	0.2734	0.0913	52 700
3/0 wire, lot No. 3......	0.4079	0.0	53 450	0.4079	0.0	53 450
	0.3457	0.0311	56 000	0.3460	0.0309	55 300
	0.3354	0.0362	55 100	0.3360	0.0359	53 300
	0.2250	0.0914	53 800	0.2263	0.0908	54 600
3/0 wire, lot No. 4......	0.4079	0.0	53 700	0.0479	0.0	53 700
	0.3454	0.0312	52 800	0.3476	0.0301	53 500
	0.3354	0.0362	54 600	0.3350	0.0364	53 000
	0.2243	0.0918	54 900	0.2264	0.0907	53 800
2/0 wire, lot No. 5......	0.3626	0.0	54 100	0.3626	0.0	54 100
	0.2994	0.0316	53 600	0.2996	0.0315	54 200
	0.2890	0.0368	53 900	0.2928	0.0349	54 500
	0.1788	0.0919	56 500	0.1814	0.0906	53 700
2/0 wire, lot No. 6......	0.3610	0.0	54 400	0.3610	0.0	54 450
	0.2990	0.0310	55 000	0.3015	0.0297	54 800
	0.2896	0.0357	55 700	0.2892	0.0359	55 300
	0.1794	0.0908	55 300	0.1810	0.0900	53 700
2/0 wire, lot No. 7......	0.3658	0.0	56 300	0.3658	0.0	56 300
	0.3506	0.0076	56 000	0.3480	0.0089	55 200
	0.3354	0.0152	56 000	0.3315	0.0171	53 640
	0.3000	0.0329	53 500	0.2985	0.0336	55 370
2/0 wire, lot No. 8......	0.3648	0.0	54 500	0.3648	0.0	54 500
	0.3000	0.0324	53 700	0.2965	0.0341	55 200
	0.2888	0.0380	54 400	0.2882	0.0383	52 300
	0.1798	0.0925	57 400	0.1802	0.0923	56 700
2/0 wire, lot No. 9...... Specially Drawn.	0.3667	0.0	52 900	0.3667	0.0	52 900
	0.2990	0.0339	54 800	0.2985	0.0341	53 700
	0.2891	0.0388	54 200	0.2900	0.0384	54 300
	0.1787	0.0940	55 500	0.1808	0.0930	55 300
1/0 wire, lot No. 10.....	0.3229	0.0	54 250	0.3229	0.0	54 250
	0.2585	0.0322	53 600	0.2645	0.0292	54 300
	0.2487	0.0371	54 800	0.2520	0.0355	53 200
	0.1293	0.0968	56 300	0.1400	0.0915	53 200
1/0 wire, lot No. 11.....	0.3234	0.0	54 400	0.3234	0.0	54 400
	0.2590	0.0322	54 700	0.2560	0.0337	53 800
	0.2494	0.0370	53 700	0.2470	0.0382	54 000
	0.1390	0.0922	53 400	0.1385	0.0924	54 000
No. 6 wire, lot No. 12...	......	......	.....	0.1618	0.0	62 550
	......	......	.....	0.0986	0.0316	61 500
	......	......	.....	0.0958	0.0330	59 600
	......	......	.....	0.0853	0.0382	61 100
	......	......	.....	0.0458	0.0580	59 800
No. 6 wire, lot No. 13...	......	......	.....	0.1619	0.0	61 350
	......	......	.....	0.0980	0.0319	60 700
	......	......	.....	0.0889	0.0365	60 800
	......	......	.....	0.0630	0.0495	60 300
	......	......	.....	0.0303	0.0658	61 000

the wire having the least amount of cold work (lowest tensile strength) as well as of the wire having the greatest amount (highest tensile strength).

In discussing these results of physical tests, let us consider briefly what happens to the equiaxed grains of a hot-rolled copper rod or annealed wire when the metal is cold drawn. The rod or wire is reduced in diameter and lengthened in proportion on passing through the tapered hole in the die. While undergoing this change the metal is subjected simultaneously to a heavy pressure by the tapering surface of the hole in the die, and to a heavy tensile stress, transmitted from the wire-drawing block through that cross-section of the wire which has already passed the die. The combination of these forces results in an amount of plastic deformation which is expressed numerically as the percentage of reduction of area by drafting. The individual grains of metal are increased in length and correspondingly decreased in transverse cross-section in proportion to the amount of drafting, the volume of each individual grain remaining practically unchanged. This change in the shape of the individual grains is accomplished by the process of slip along certain of its crystallographic planes with the resulting amorphitization of metal adjacent to these planes.

The microscopical examination of longitudinal sections readily shows the change in grain dimensions above described. Such examination further indicates that plastic deformation has progressed equally throughout the section of the drawn wire. From this evidence alone we should anticipate that all points in the wire, regardless of their position in relation to the long axis of the wire, would show similar increases in hardness and tensile strength as a result of the plastic deformation.

Looking at the matter from another point of view, we must assume that if the inner portions of the wire were to acquire less hardness than the outer portions, those inner portions must undergo less plastic deformation (increase in length) in drafting than the outer portions; in other words, the outer portion of the wire would necessarily increase in length at a faster rate than the inner portions. This condition would result in a finished wire that would be a tube instead of solid on the end last leaving the die, and in fact would be a tube for a considerable proportion of its length. This condition does not exist, in spite of the fact that the wire may be increased to nearly twice its original length in a single draft. Even a copper-clad rod (which consists of a jacket of soft copper welded to a core of comparatively hard steel) can be drawn down into a small-diameter wire in which the steel center has increased in length to the same extent as the copper. Since the inner portions actually increase in length by the same amount as the outer portions, they must have undergone similar amounts of plastic deformation, and therefore, have increased in hardness and tensile strength to the same degree as the outer portions.

CONCLUSION.

From the data presented and from a theoretical consideration of the nature of the process of cold drafting, the conclusion is warranted that hard-drawn copper wire is equally affected throughout its mass by the cold-drawing process, and hence, that the common conception of a hard, exterior skin is erroneous.

DISCUSSION.

Mr. Addicks. MR. LAWRENCE ADDICKS (*presented in written form and read by the Secretary*).—There is ample evidence in the behavior of copper wire under torsion to demonstrate the existence of a hard skin of appreciable thickness on ordinary copper wire. It is not of sufficient thickness to affect the tensile strength to any extent, but the removal of a very thin film of the outside surface by etching with acid has a surprising effect on the torsional resistance. As the similar removal of some of the surface of the rod before drawing the wire has an almost equal effect on the finished wire, it seems probable that the hardness is due to oxide scale rolled in rather than to the actual drawing and this therefore refers only to commercial wire.

It should not be difficult by actual test to measure both the thickness and the hardness of this layer from torsional data.

Mr. Howe. MR. HENRY M. HOWE.—This paper is very welcome as showing that the effect of cold drawing is not concentrated on the surface of the piece but is approximately uniform throughout the cross-section. This is of both scientific and practical importance.

If we analyze the data in his Table I as I have done in Table III, we may draw two interesting inferences. First we note that the hardness of the polished skin exceeds that of the drawn skin in four out of the five cases by from 2 to 5 Brinell numbers, from which we infer that the polishing has added slightly to the effect of the cold drawing. This is in exact conformity with Beilby's observation, which showed that the effect of polishing is to create a wholly amorphous surface layer, not only appreciably harder than the normal metal, but transparent and differing from it in the physical properties in general.

The differences shown in Table III in this respect are very small, but the fact that the polished skin in four out of the five cases is measurably harder and in the fifth case is not softer than the drawn skin should, I think, be interpreted first as

Mr. Howe.

confirming Beilby's observations, and second, as testifying to the accuracy of Mr. Peirce's work.

In all cases except No. 1 the polished cross-section is somewhat harder than the polished skin, and in all the cases except No. 3 the center of the polished cross-section is somewhat harder than the peripheral parts. The difference is somewhat larger in this case than in the previous one. This strongly tends to show that the hardening increases slightly from the periphery toward the axis. This is further supported by the fact that the total increase of hardness from No. 1 to No. 5 is 29.3 Brinell numbers in the cross-section against 16 Brinell numbers in the polished skin.

Table III.—Analysis of Brinell Hardness Data in Table I.

Specimen No.	Polished Cross-Section.	Polished Skin.	Drawn Skin.	Excess of Polished Cross-Section over Polished Skin.	Excess of Polished Skin over Drawn Skin.
1	101.7	103	101	−1.3	2
2	120	108	104	12	4
3	116	105	103	11	2
4	128	122	117	6	5
5	131	119	119	12	0

There is an anomaly which Mr. Peirce may be able to explain to us, that the hardness increases from draft to draft from specimen No. 1 to specimen No. 5 whether we compare the polished cross-section, the polished skin, or the drawn skin, with the exception of specimen No. 3, which is softer than specimen No. 2 in all three respects in spite of having been drawn more. Moreover, if we compare the individual cases Nos. 1 to 7 of specimen No. 2 with Nos. 1 to 8 of specimen No. 3 (omitting No. 6 of specimen No. 3, which is without any strict correspondent in specimen No. 2), we find that in six out of the seven positions specimen No. 2 is harder than specimen No. 3, and in the seventh position their hardness is equal. There is also a small decrease in hardness of the polished skin from specimen No. 4 to specimen No. 5, but this must be taken as within the limit of observational error.

Mr. Howe. Mr. Peirce's explanation of the uniformity of the hardening throughout the cross-section is complete and convincing.

His results confirm those which Thurston reached in 1878,[1] on examining a cold-rolled wrought-iron bar, and testing tensilely pieces cut from it, some of them of the full 2-in. diameter and others reduced in diameter by turning in a lathe to various diameters down to $\frac{1}{4}$ in. His data, like Mr. Peirce's, show that the great strengthening effect produced by the cold rolling is substantially uniform throughout the cross-section of the bar. His results are less concordant than Mr. Peirce's, which is hardly surprising in view of the heterogeneousness of the wrought iron, and further, in view of the fact that his specimens do not appear to have been homologous, but to have had constant length in spite of the varying cross-section.

The greater hardness of the cross-section than of the skin may perhaps be due to the hardening effect of cutting the cross-section, for instance by the hack saw if that was used.

Mr. Peirce has kindly enabled me to reproduce his miniature Brinell machine for use in my own laboratory. It is not only very cheap in construction, but has some marked advantages over the standard machine, in that normal impressions can be had closer together, nearer the edge, and on probably much thinner sheets of metal.

The machine has, of course, the defects of its quality. It enables us to detect irregularities of hardness, and thus to test the material for irregularity of composition, better than with the standard machine. On the other hand, if we seek the average hardness of a heterogeneous material as a whole, we have to make and average a larger number of impressions than with the standard machine.

(By letter).—While it is true that the greater hardness of the cross-section might be referred to the longitudinal distribution of the structure or of impurities, yet certain objections to this occur. In the first place even in the "commercial" copper wire tested the proportion of impurities is probably almost negligibly small. In the second place I hardly see why the ends of the elongated crystals should offer greater resistance than the sides.

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[1] R. H. Thurston, Report to Jones and Laughlin Steel Co. on Cold-Rolled Iron, 1878. See also Howe, "Metallurgy of Steel," page 216, 1890.

Mr. Howe.

Would not a Brinell impression be greater on the end than on the side of a wire rope?

There was no clear indication of a difference in hardness between the transverse and the longitudinal sections of the torn tensile test piece of low-carbon steel which I described[1] in 1914, though in this case the other variables were so great that such a difference might exist and yet escape unnoticed.

A more attractive explanation of the greater hardness of the core in Mr. Peirce's rods is that the core is under initial compression and the skin is under initial tension, as was shown by Heyn[2] in the case of drawn rods, the opposite being true of rolled and hammered rods. It is easy to understand that the fact that the core is initially in compression increases resistance to the intrusion of a foreign body like the Brinell ball. Because the core is under compression it must needs pass on to the layers which compress it a part of the compression which it receives from the Brinell ball, so that to the resistance of the core itself to compression there is added the resistance which that compression has to overcome in moving radially toward the layers which are compressing it.

It would be interesting if Mr. Peirce would next proceed to try this same experiment with rolled rods, and if he would also slit his rods lengthwise and see whether the two halves become concave outwards.

The Brinell impression seems to be brought about chiefly by moving the material at and near the surface radially and upwards, so that the resistance to impression is chiefly tensile. It is only when the tensile elastic limit is exceeded that such tensile movement is brought about. If the metal is initially in compression, that compression increases the external force which must be applied to bring the material up to its tensile elastic limit. Hence the initial compression lessens the excess above the tensile elastic limit which the fixed pressure on the Brinell ball causes, and in lessening that excess lessens the size of the resultant impression, and thus increases the hardness number.

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[1] Henry M. Howe, "Are the Effects of Overstrain Monotropic," *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part II, p. 28 (1914).

[2] Martens-Heyn, "Materialenkunde," 1912, p. 301.

Mr. de Forest. MR. A. V. DE FOREST.—In reply to Mr. Howe's suggestion of the small Brinell machine, I have had a little experience with a 1-mm. ball and 10-kg. load, which is fairly small for the Brinell test. The limit of usefulness of that machine is pretty nearly dependent on the grain size of the specimen which is being examined; because if the ball happens to fall on a junction of three grains, it will give an entirely different reading from what it will give if it happens to fall in the center of one grain; and when the depression is so very small, it is easy for this to occur, and it leads to extreme confusion in interpreting the results.

Mr. Campbell. MR. WILLIAM CAMPBELL.—Two or three years ago I had some hard-drawn wire which I obtained through the kindness of Mr. J. A. Capp, to examine the structure. I wanted to determine whether there was any difference in the structure, and probably in the hardness, between the outside and interior of cold-drawn metal. Up to 500 diameters you could not tell the difference between the photographs taken at the outside and the inside. I did not have the facilities for making tensile tests or these Brinell tests with the small ball, and I think we are greatly indebted to Mr. Peirce for having done this work and cleared up, I hope once for all, the certainly erroneous idea that cold-drawn metal is harder and stronger on the outside than on the inside.

In regard to the reason why there should be a difference in hardness between the end and the side, I think that has something to do with the structure. If the metal is examined longitudinally, you will find that it is composed of a bunch of drawn-out fibers or elongated grains. Pressing the ball upon it in from the sides tends to shove those elongated grains aside. On the other hand, looking at it in a cross-section, you will find that you are examining the ends of a number of rods, as it were. Now on pressing that structure with the ball, you are not doing exactly the same work as before; it is somewhat similar to trying to push your finger sideways on a rope and endways on a rope.

Mr. Peirce. MR. E. H. PEIRCE (*Author's closure by letter*).—Mr. Addicks believes that there exists on ordinary copper wire a hard skin of appreciable thickness, but considers it probable that this is due to oxide scale rolled in rather than to the actual drawing.

Mr. Peirce.

The existence of a hard skin of appreciable thickness was certainly not indicated by the hardness tests obtained in my experiments. Using various specimens of commercial copper wire and a modified Brinell test in which the ball penetrated the surface less than 0.002 in., the hardness of the drawn surface was not higher than that of the polished surface nor that of the polished sections. An immeasurably thin layer of hard amorphous metal undoubtedly exists on the surface of hard-drawn wire. The effect on the torsion test of wire due to treatment of the surface of wire (or even rods from which wire is drawn) with dilute nitric acid can, however, hardly be claimed to demonstrate the existence of a "hard skin of appreciable thickness" when hardness tests fail to show that the surface of either rods or wire is actually harder to an appreciable depth.

Mr. Howe calls attention to the fact that the drawn surfaces show slightly lower hardness than similar surfaces which have been polished. An immeasurably thin layer of amorphous material forms the surface of the drawn wire, while the amorphous layer formed by the polishing is doubtless thicker than that of the drawn surface, and therefore, gives a higher hardness figure in the test. Mr. Howe notes in his Table III a slight excess of the hardness of the polished cross-section over the polished surface of the wire and suggests that the cutting of the cross-section with the hack saw may have hardened the cross-section tested. I agree that this sawing would harden the metal adjacent to the cut. He further refers to Heyn's conclusion that the core is under initial compression and the surface under initial tension and shows why higher hardness figures are to be expected from those portions under initial compression. In addition to the above possible causes suggested by Mr. Howe for the increased hardness of the cross-section over that of the surface, the writer believes that there is still another condition which contributes to the slight difference in hardness noted. This condition is that in taking the hardness of the polished cross-section the ball acts in a direction parallel to that of the drafting, while in taking the hardness of the polished surface of the wire the ball acts in a direction perpendicular to that of the drafting. We have recently made hardness tests which demonstrate that when testing hard-drawn wire this change of direction of the

Mr. Peirce. Brinell impression is of itself the cause of a slight difference in the hardness readings. These tests were made near the axis of the wire, both parallel to and perpendicular to the axis. The specimens tested were sawed polished cross-sections and sawed polished longitudinal sections made by cutting along the line of the axis of the wire. In these tests the hardness of the cross-section (that is, with the ball acting parallel to the direction of drafting) averaged several points higher than that of the longitudinal section (that is, with the ball acting perpendicularly to the direction of drafting). The several causes noted above probably contribute to make the hardness of the cross-section read slightly higher than that of the surface of the wire. The excess hardness under discussion is, however, quite small when we consider that the wire has increased in hardness by drafting from about 50 in the rod to 100 or higher in the drawn wire.

As suggested by Mr. Howe, a sample of a straight piece of hard-drawn copper wire was slit lengthwise with a hack saw to see whether the two halves would concave outwards. One half concaved outwards very slightly; the other half showed no change.

Mr. Howe notes that in Table I, specimens Nos. 3 and 5 did not increase in hardness over the former draft as did the other specimens. The author calls attention to the fact that these specimens had a much lighter reduction from the next larger size than had specimens Nos. 2 and 4. These light reductions would be expected, however, to produce slight increases in hardness over the former draft.

Mr. Campbell explains why there should be a difference in hardness between the end and side of a wire. I agree that the end would be expected to give a slightly higher hardness figure than the side. The hardness tests made near the axis of a wire, both parallel and perpendicular to the axis, as described in my reply above to Mr. Howe's discussion, confirm this view.

ELECTROLYTIC DETERMINATION OF TIN ON TINNED COPPER WIRE.

By G. G. Grower.

SUMMARY.

As it is sometimes necessary to determine the quantity of tin on tinned copper wire, a quick method of making the determination was desired. Where two different metals are in intimate contact, as in plated material, it would appear that electrolysis would offer a ready means of separating and determining the two constitutents. The method was tried with tinned copper wire and found to be practical. The investigation also showed that the tin exists in two forms, free tin on the outside, and an intermediate layer of tin-copper alloy. On account of the difference of potential between the free and alloyed tin, it is possible to determine these separately. Gas voltameters are used to show the quantity of tin and no weighing is necessary. The electrolysis only takes a minute or so.

The method may be used to determine the quantity of free and alloyed tin, but is not an accurate indication of the uniformity or perfection of tinning.

ELECTROLYTIC DETERMINATION OF TIN ON TINNED COPPER WIRE.

By G. G. Grower.

General Description of Process.

Introduction.—The quantity of tin on tinned copper wire has hitherto been determined by chemical analysis. This is inconvenient, being slow, and necessitating sending the wire to the chemical laboratory. The analysis shows only the quantity of tin, and gives no information concerning its condition or uniformity. Other methods are, therefore, desirable to show the quantity or quality of the tinning: for example; the wire has been alternately dipped in solutions of hydrochloric acid and sodium sulfide, the number of such cycles without blackening is taken as showing in a general way how good the tinning is. The method is unsatisfactory and the results of doubtful value. Exposure of the wire to hydrogen-sulfide gas has also been used as a test. This method is difficult and still less satisfactory than the other.

An electrolytic method is here suggested which, although far from perfect, appears to be much better than anything previously suggested. The test can be made in a couple of minutes, is fairly accurate, and gives at least some information concerning the quality of the tinning. The apparatus about to be described is suitable for moderate sizes of tinned wire, say from No. 8 to No. 24 B. & S. gage.

Structure of Tinned Copper Wire.—Commercial tinned copper wire is produced by passing the cleaned copper wire through a bath of melted tin, and removing the excess of tin by means of a wipe. Wire thus produced invariably consists of three parts; the inside copper, the outside tin, and between the two a layer of copper-tin alloy.

The inside copper is simply well-known copper wire and needs no further description.

The outside tin or free tin will usually be present in amounts between one-half and two milligrams per square centimeter of surface. It is far from uniform, varying greatly from place to place on the same coil and even on different diameters at the same place. Very often the thicker portions are found in spots or streaks running in lines along the length of the wire, evidently due to irregular action of the wipe. Beside these greater irregularities just referred to, the whole surface appears to be mottled or speckled, as if the tin were in little blotches of varying thickness. This may be due to the crystallin structure of tin.

The study of the layer of alloy is a difficult problem, and not much is definitely known about it. It is produced by the contact of copper and melted tin, for it is found that samples of wire which are tinned by electro-plating instead of passing through the melted tin, are free from this layer, and consist simply of the inside copper and the outside tin. It is invariably found in the tinned wire of commerce. Whether this layer is of definite composition throughout, or consists of a series of thinner layers of varying composition is not known definitely; there are some reasons for believing it to have the composition Cu_3Sn, and for the purpose of this test it may be considered definite in composition. It will be spoken of as the alloy, and the tin contained in it will be called the alloyed tin to distinguish between this and the free tin on the outside of the wire. The quantity of alloyed tin is usually less than that of the free tin. The alloy is dark gray in color. It is found to be electro-negative to tin by about 0.5 volt in dilute sulfuric acid and some other electrolytes. This is also the position of copper, so that in the electrochemical series copper and the alloy are about equal. It is upon the electrochemical properties of the substances that the method of testing about to be described is based.

The total quantity of tin on the wire, as given by chemical analyses, will, of course, be the sum of the free tin and the alloyed tin.

The Electrolytic Test.—If a glass containing dilute sulfuric acid has one terminal of platinum wire and another of tinned copper wire, and an electric current be passed through it from the tinned wire to the platinum, hydrogen gas will be evolved at the platinum, and tin will dissolve off the tinned wire. By Faraday's law the amount of chemical action at each terminal will be proportional to the quantity of electricity which passes.

As the tin is dissolved off the wire alloy is exposed, but since tin is electro-positive to the alloy no alloy will be dissolved as long as any appreciable amount of tin is left. If at the moment all the free tin is dissolved, and before any of the alloy is affected, the quantity of electricity which has passed is noted, we obtain at once a measure of the quantity of free tin which was on the

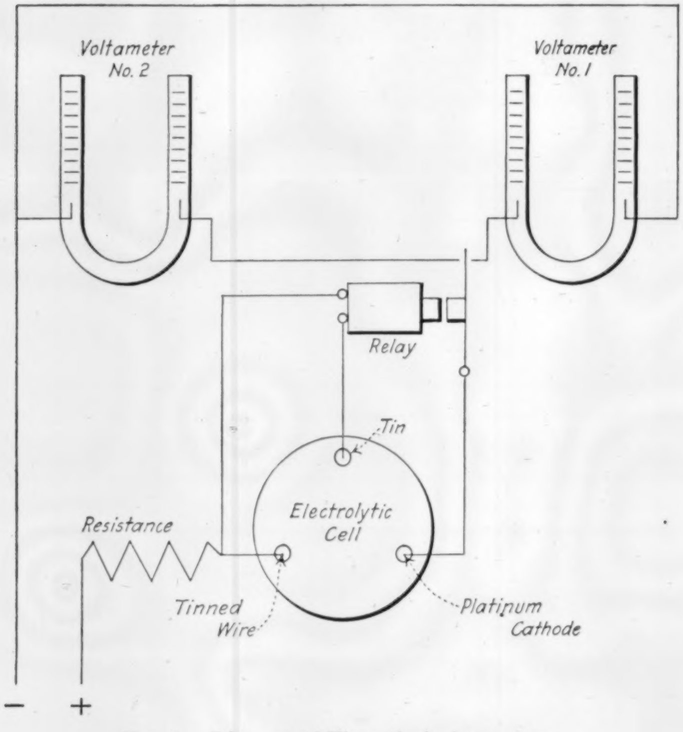
Voltameter No. 2
Voltameter No. 1
Relay
Tin
Electrolytic Cell
Resistance
Tinned Wire
Platinum Cathode
–
+

FIG. 1.—Diagram of Electrolytic Apparatus.

wire. The wire in the solution will now be dark gray in color, showing the layer of alloy. If the electric current be continued this alloy will begin to dissolve, and presently the reddish color of copper begins to show. This color gradually becomes richer and richer as the alloy is dissolved, until finally the pure copper alone remains. If now the additional quantity of electricity which has passed be noted, we have a reading which must have some relation to the quantity of alloy. Just what this relation is I am not prepared to say, but evidently greater readings mean greater quantity of alloy, and many experiments show that these readings can be repeated again and again with what might be considered fair agreement under the circumstances.

If the electrolytic current be further continued copper will dissolve, but this is of no interest, for the tin has been completely removed.

The Apparatus.—It is convenient to use two gas voltameters for measuring the quantity of electricity; one for that used in dissolving the free tin, the other for that used in dissolving the alloy. A relay may be used to shift the circuit automatically at the proper instant by connecting one of its magnet terminals to the tinned wire and the other to a neutral electrode of tin which also dips into the acid. The action is as follows:

The tin and the tinned wire to which the magnet terminals of the relay are connected both dipping in the sulfuric acid constitute a voltaic cell acting upon the relay. Experiment has shown that as long as there is any appreciable quantity of free tin on the wire it acts as if it were all tin, and we have an electromotive-force of tin and tin (that is, tin electrode and tinned wire). Both terminals being alike, no current acts on the relay and the armature is held back by the spring closing circuit No. 1. But as soon as the free tin is all removed we have an electromotive-force of tin and alloy, about half a volt acting on the relay; the magnet attracts the armature and closes circuit No. 2. Fig. 1 shows the arrangement.

In the practical instrument a double contact key or similar device should be used so that the relay circuit is not closed until after the tinned wire is dipped into the acid, and is opened before it is removed, otherwise the relay will be injured, or at least thrown out of adjustment when the tinned wire is removed. The platinum terminal also gradually receives a deposit of copper and tin mostly in a very loose, spongy form. It is well to put a reversing switch in the circuit, so that this platinum can be cleaned occasionally by reversing the current. Fig. 2 shows the connections for the practical instrument.

The voltameters can be designed to measure either hydrogen, oxygen, or the mixture of the two. In an instrument used by the writer, shown in Fig. 3, the same voltameters could be used in all three ways by means of extra terminals and switches, but this is not to be recommended. This form of voltameter has, however, proved very convenient for the work.

After a reading has been taken it is only necessary to open the stop-cock for a few seconds allowing the gas to escape; the tube is refilled automatically, and all is ready for another reading. The graduated tubes are 5 cc. in volume, the graduations being 0.02 cc.; hundredths are easily estimated, and the graduated portions are about 24 cm. long.

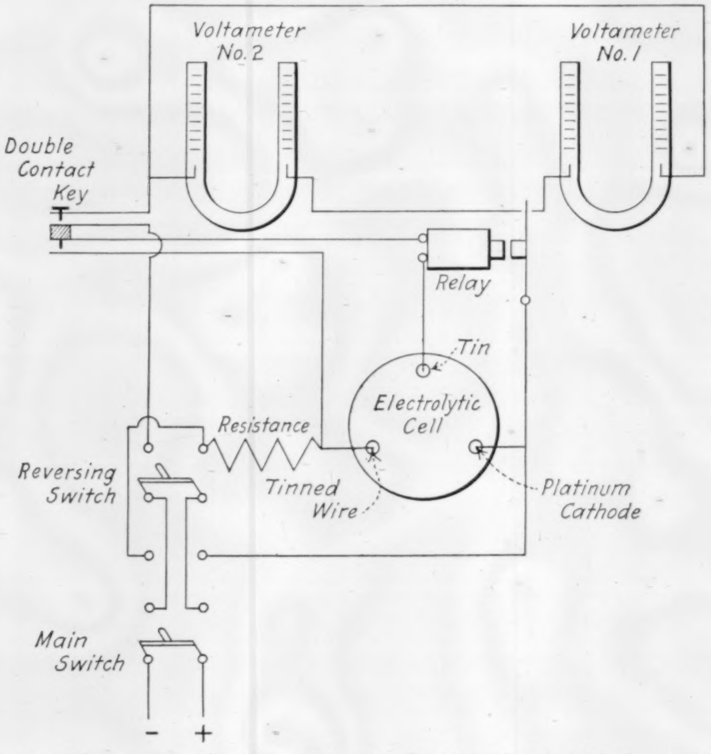
Voltameter No. 2
Voltameter No. 1
Double Contact Key
Relay
Tin
Electrolytic Cell
Resistance
Reversing Switch
Tinned Wire
Platinum Cathode
Main Switch
-
+

FIG. 2.—Diagram Showing Connections for Practical Electrolytic Apparatus.

Adjustments.—Connect the apparatus as shown diagrama-

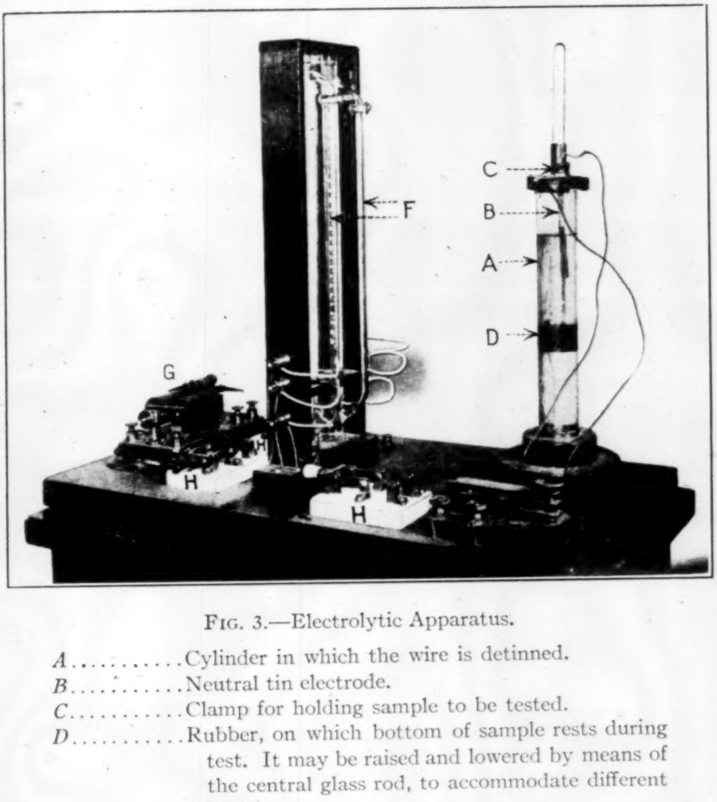

FIG. 3.—Electrolytic Apparatus.

A	Cylinder in which the wire is detinned.
B	Neutral tin electrode.
C	Clamp for holding sample to be tested.
D	Rubber, on which bottom of sample rests during test. It may be raised and lowered by means of the central glass rod, to accommodate different lengths.
E	Double contact key, which closes the relay circuit after the other circuit has been completed.
F, F	Voltameters.
G	Relay.
H, H, H	Double-throw switches.

tically in Fig. 2. The electrolyte used in both voltameters and the cylinder for dissolving the tin may be made of one volume of c. p. concentrated sulfuric acid and 10 volumes of water. Fill both voltameters and open the stop-cocks, also put enough dilute acid in the cylinder so that 10 cm. of wire will be immersed.

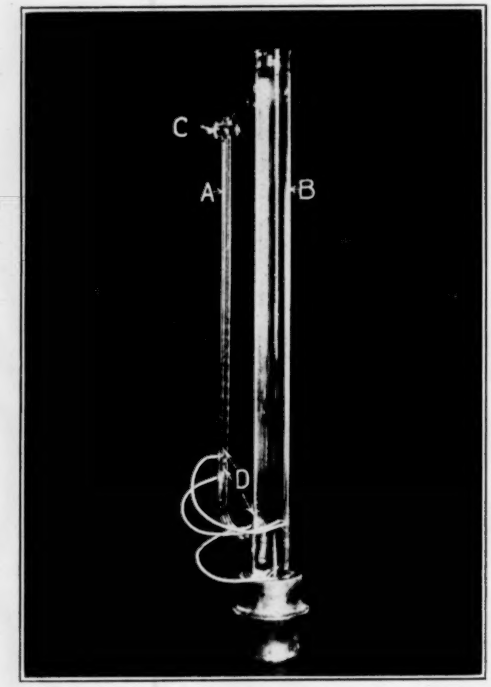

FIG. 4.—Voltameter.

A	Graduated Gas Burette.
B	Acid Reservoir.
C	Glass stop-cock.
D, *D*, *D*	Platinum Electrodes.

Have the resistance such that 0.2 or 0.3 ampere will be the current for testing moderate size wires, say 10 to 16 B. & S. gage. Put a piece of tinned wire in the clamp over the cylinder so that it dips into the solution. Close the main switch and press the key.

The relay should be adjusted so that while there is free tin on the wire the spring pulls the armature back and closes circuit No. 1, but when only alloy or copper is exposed the magnet pulls the armature forward closing circuit No. 2. If the relay is a good one and suitable, the adjustment will not be difficult.

After these adjustments have been made, and once made they may last a long time, all is ready for making the test.

Making the Test.—Close the stop-cocks in the voltameters, insert the wire to be tested in the clamp so that it dips into the solution, and press down the double contact key. Immediately gas will begin to form in voltameter No. 1 as the free tin is dissolving off the wire. In a minute more or less the free tin will be dissolved, the relay will operate, and gas will be produced in voltameter No. 2. The sample of wire will now be dark gray in color. Watch it carefully, gradually the reddish copper begins to show, and this color becomes richer and richer. As long as the color is deepening it shows that the alloy is not all removed. When the color ceases to increase in intensity the key is released, the sample of wire removed, and readings on the voltameters are taken. The stop-cocks are then opened for a second or two, closed again, and all is ready for another test.

The same electrolyte may be used for a large number of determinations.

If V_1 be the number of cubic centimeters of hydrogen gas produced in voltameter No. 1, V_2 in voltameter No. 2, and d the diameter of the wire in mils, we have for the number of milligrams of tin per square centimeter of surface (a 10-cm. length being tested)

$$\text{Free Tin} = \frac{62\ V_1}{d} \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

$$\text{Alloyed Tin} = \frac{30\ V_2}{d} \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

and the total tin will be the sum of these two. Eq. 1 is calculated from the fundamental physical and chemical constants. Eq. 2 is arbitrarily taken so as to make the total tin agree as well as possible with that obtained by chemical analysis. No claim for exactness is made for Eq. 2; it is only an approximation.

In regard to the quantity of alloyed tin, it must be stated here that the end point is not very definite, and there is a chance for a difference of opinion of several seconds as to when the alloy is all removed and the clean copper exposed. Sometimes one part of the wire will be clean copper, while another part will still plainly show alloy on it. All we can do in such cases is to make the best compromise we can. It will be seen that the results cannot be expected to be very accurate for alloyed tin.

TABLE I.—QUANTITY OF TIN ON TINNED COPPER WIRE.

(RESULTS OF CHEMICAL ANALYSES.)

Size, B. & S.	Diameter, in.	Number of Coils Analyzed.	Tin, per cent.			Milligrams per sq. cm., average.	Analysis of Large Sample. (about 10 lb.)		Analysis of Different Samples from One Coil, per cent.
			Max.	Min.	Aver.		Per Cent.	Milligrams per sq. cm.	
0000	0.460	7	0.11	0.05	0.08	2.10	0.10	2.62	....
0	0.325	7	0.12	0.05	0.08	1.48	....	....	....
2	0.258	..	....	....	....	....	0.13	1.91	....
4	0.204	10	0.21	0.10	0.13	1.51	0.14	1.62	....
6	0.162	..	....	....	....	....	0.17	1.57	....
8	0.128	10	0.31	0.13	0.18	1.31	0.22	1.61	....
10	0.102	..	....	....	....	....	0.20	1.16	....
12	0.081	10	0.36	0.22	0.29	1.34	0.23	1.06	(0.36 - 0.37) (0.22 - 0.25)
14	0.064	10	0.45	0.26	0.35	1.28	0.35	1.28	(0.37 - 0.45) (0.29 - 0.30)
16	0.051	11	0.58	0.23	0.38	1.10	0.40	1.16	(0.23 - 0.30) (0.58 - 0.58)
18	0.040	..	....	....	....	....	....	....	(0.41 - 0.41 - 0.36 - 0.61)
20	0.032	10	1.29	0.35	0.79	1.44	....	....	....
24	0.020	10	3.01	0.78	1.41	1.62	....	....	(0.54 - 1.85)
28	0.013	10	3.51	0.97	2.15	1.54	....	....	....
30	0.010	..	....	....	....	....	....	....	(0.99 - 1.08)
32	0.008	10	1.49	0.79	1.06	0.48	....	....	....
36	0.005	10	1.88	0.89	1.33	0.38	....	....	....
40	0.003	10	4.14	1.38	2.72	0.48	....	....	....

Quantity of Tin on the Wire.—Table I shows the analytical results obtained from various sizes of tinned wire. They are given to show about what might be expected in practice.

The results headed "Analysis of large sample" were obtained by melting about 10 lb. of the wire and then analyzing a sample of the resulting alloy. The figures coupled in parentheses, under the heading "Analysis of different samples from one coil," show the analysis of two or more different samples from the same coil. It will be seen that the quantity of tin may vary considerably from one portion of the coil to another. For example, on size No. 24 one sample showed about three and one-half times as much tin as the other, both being taken from one coil.

EXPERIMENTAL RESULTS.

The Electrolyte.—It would appear at first that a great number of electrolytes might be used with more or less success for the electrolytic determination of tin; but when tried, we find many of them are unsuitable for one or more reasons. Hydrochloric and nitric acids attack tin, and are therefore unsuitable. Substances like potassium hydroxide, potassium cyanide, potassium binoxalate, and oxalic acid are found to liberate gas at both poles, and therefore could not give quantitative results. Other substances such as sodium chloride are perhaps quantitative in their results, but they rapidly become turbid, so that the wire cannot be seen; a small quantity of oxalic acid added to the solution will keep it clear. Dilute sulfuric acid works well.

Most of the experimental work was done either with dilute sulfuric acid (one part of acid to ten of water by volume, unless otherwise stated) or sodium chloride solution (water 100 parts, sodium chloride ten parts, oxalic acid one part by weight). Apparently dilute sulfuric acid is most suitable and has therefore been recommended.

Dilute sulfuric acid does not attack tin to any great extent, as the following experiment will show:

Two pieces of tin wire were scoured with crocus cloth, dipped one minute in dilute sulfuric acid (1 : 10 by volume), washed with water and then with alcohol. The alcohol was ignited, and when cool the wires were weighed. They were then put back into the acid for ten minutes, washed with water and alcohol and again weighed. The results were as follows:

	No. 1	No. 2
After one minute in acid, grams	2.7719	2.7431
" ten minutes more in acid, grams	2.7717	2.7430
Loss, grams	0.0002	0.0001

The Electro-Chemical Equivalent.—The electro-chemical equivalent for stannous tin is 0.0006166 grams per coulomb. The following determinations were made to ascertain whether or not the action under consideration agreed with this law.

Eight determinations were made on two different days; two with dilute sulfuric acid (1 : 10 by volume); two with dilute sulfuric acid (1 : 50 by volume); two with the sodium-chloride solution referred to above; and two with one volume of this solution diluted with four volumes of water. The current used was approximately one-tenth of an ampere, and was left on for ten minutes; readings were taken on a Weston ammeter at the end of $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$ minutes and so on throughout the experiment. Two solutions were run in series, one containing sulfuric acid, the other containing sodium chloride.

The results in detail were as follows:

No. 1.—STARTED AT 1.33 P. M.

Reading at	$\frac{1}{2}$ minute	19.8
"	$1\frac{1}{2}$ "	19.7
"	$2\frac{1}{2}$ "	19.8
"	$3\frac{1}{2}$ "	19.5
"	$4\frac{1}{2}$ "	19.4
"	$5\frac{1}{2}$ "	19.3
"	$6\frac{1}{2}$ "	19.3
"	$7\frac{1}{2}$ "	19.2
"	$8\frac{1}{2}$ "	19.2
"	$9\frac{1}{2}$ "	19.3
	Average	19.45

Average Amperes	0.09725
Coulombs	58.33
Calculated loss	0.0360 g.

Electrolyte: Sulfuric Acid, 1 : 10.

Weight of Anode, before	2.7025 g.
" " " after	2.6667 g.
Loss	0.0358 g.

No. 1.—*Continued.*

Electrolyte:		
	Sodium Chloride.....	10 parts
	Oxalic Acid........	1 "
	Water..............	100 "

Weight of Anode, before........................	2.6540 g.
" " " after (wiped off)..............	2.6169 g.
Loss..................................	0.0371 g.

No. 2.—STARTED AT 2.55 P. M.

Reading at	½ minute	19.5
"	1½ "	19.2
"	2½ "	18.9
"	3½ "	18.9
"	4½ "	19.0
"	5½ "	19.0
"	6½ "	18.9
"	7½ "	19.0
"	8½ "	19.0
"	9½ "	18.8
	Average	19.01

Average Amperes..............................	0.09505
Coulombs....................................	57.03
Calculated loss..............................	0.0352 g.

Electrolyte: Sulfuric Acid, 1 : 50.

Weight of Anode, before........................	2.6150 g.
" " " after........................	2.5789 g.
Loss..................................	0.0361 g.

Electrolyte: Sodium chloride solution diluted with four volumes of water.

Weight of Anode, before........................	2.6635 g.
" " " after (wiped off)..............	2.6267 g.
Loss..................................	0.0368 g.

No. 3.—Started at 2.04 p. m.

Reading at ½ minute	21.3
" 1½ "	20.7
" 2½ "	20.8
" 3½ "	20.6
" 4½ "	20.5
" 5½ "	20.4
" 6½ "	20.5
" 7½ "	20.2
" 8½ "	20.0
" 9½ "	20.0
Average	20.50

Average Amperes	0.1025
Coulombs	61.50
Calculated loss	0.0379 g.

Electrolyte: Sulfuric Acid 1 : 10.

Weight of Anode, before	2.7725 g.
" " " after	2.7345 g.
Loss	0.0380 g.

Electrolyte:

Sodium Chloride	10 parts
Oxalic Acid	1 "
Water	100 "

Weight of Anode, before	2.7434 g.
" " " after (wiped off)	2.7061 g.
Loss	0.0373 g.

No. 4.—Started at 4.20 p. m.

Reading at ½ minute	21.1
" 1½ "	20.6
" 2½ "	20.2
" 3½ "	20.3
" 4½ "	20.2
" 5½ "	20.2
" 6½ "	20.0
" 7½ "	19.8
" 8½ "	19.8
" 9½ "	19.9
Average	20.21

Average Amperes	0.10105
Coulombs	60.63
Calculated loss	0.0374 g.

No. 4.—*Continued.*

Electrolyte: Sulfuric Acid 1 : 50.

Weight of Anode, before	2.7316 g.
" " " after	2.6939 g.
Loss	0.0377 g.

Electrolyte: Sodium chloride solution diluted with four volumes of water.

Weight of Anode, before	2.7036 g.
" " " after (wiped off)	2.6688 g.
Loss	0.0348 g.

When the experiment was made in the sodium chloride-oxalic acid solution, a fine, crystallin deposit of metallic tin was found upon the surface of the anode, and this had to be wiped or scraped off before weighing. If it was left on, the loss of weight in the strong solution was only about one-half of the calculated value. I do not, at present, understand the cause of this deposit. It did not occur in the sulfuric acid solution.

There was also a loose, crystallin deposit of metallic tin at the lower end of the cathode in all cases. After passing the current, both solutions were found to contain stannous tin. Summarizing results, the losses are as follows:

In 1 : 10 Acid.		In Strong Sodium Chloride.	
Calculated.	Observed.	Calculated.	Observed.
0.0360	0.0358	0.0360	0.0371
0.0379	0.0380	0.0379	0.0373
In 1 : 50 Acid.		In Diluted Sodium Chloride.	
0.0352	0.0361	0.0352	0.0368
0.0374	0.0377	0.0374	0.0348

It will be seen that the results with sodium chloride are quite irregular, possibly due to the crystallin deposit referred to above. The results with the stronger acid are, perhaps, as good as could be expected; with the weak acid they are not so good.

Strength of Solution.—The strength of the solution may be varied between relatively large limits, but it has been found that the limit of permissible current depends upon the strength of the solution. This will be further discussed in the section on electrolytic phenomena.

Table II gives the results obtained with sulfuric acid of several different strengths. All the readings were taken from one coil of 0.064-in. tinned copper wire, the current being 0.2 ampere, and the length tested 10 cm. The figures show the number of cubic centimeters of hydrogen gas evolved.

TABLE II.—CUBIC CENTIMETERS OF HYDROGEN GAS EVOLVED FROM 10 CM. OF WIRE.

Strength of Sulphuric Acid by Volume.

	1 : 5		1 : 6		1 : 7		1 : 8		1 : 12		1 : 20	
	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.
	0.68	0.76	0.65	0.72	0.71	0.72	0.67	0.71	0.63	0.82	0.64	0.76
	0.59	0.74	0.68	0.68	0.57	0.86	0.67	0.76	0.64	0.85	0.68	0.75
	0.56	0.90	0.56	0.81	0.68	0.90	0.64	0.77	0.68	0.78	0.71	1.02
	0.58	0.74	0.61	0.87	0.78	0.92	0.60	0.82	0.65	0.82	0.64	0.99
	0.54	0.73	0.58	0.80	0.66	0.80	0.58	0.81	0.59	0.71	0.64	0.69
	0.53	0.73	0.63	0.75	0.60	0.86	0.56	0.90	0.65	0.70	0.58	0.72
	0.69	0.69	0.71	0.89	0.76	0.85	0.70	0.90	0.87	0.72	0.53	1.05
	0.68	0.89	0.66	0.80	0.76	0.80	0.71	0.92	0.90	0.70	0.70	0.72
	0.80	0.78	0.66	0.85	0.54	0.74	0.64	0.88	0.59	0.79	0.63	0.96
	0.64	0.75	0.61	0.83	0.61	0.89	0.63	0.94	0.56	0.88	0.62	0.82
Average	0.627	0.771	0.635	0.800	0.667	0.834	0.640	0.841	0.676	0.777	0.637	0.844

Current Density.—The current density, that is, the current per square centimeter of surface, must not be too great. If it is too great gas will be given off at the anode, and certain complicated results which are not properly understood will be obtained. These will be referred to in the next section.

If the current density is not too great, the current strength does not appear to affect the results, as the figures in Table III will show. They give the number of cubic centimeters of hydrogen gas from samples off the same coil, referred to in the preceding section. The strength of current was varied from 0.1 to 0.5 ampere.

Electrolytic Phenomena.—The phenomena accompanying the electrolysis are not understood. They are complicated and difficult to describe.

If the current density is not too great the tin usually begins to lose its luster and the color merges into the dark gray of the alloy, but frequently a more or less sudden change in the appearance occurs, sometimes becoming darker and sometimes lighter. Occasionally a distinct darkening of the surface occurs just as the relay works, corresponding to the removal of the last of the free tin. When the tin is in streaks or spots it is frequently possible, by watching these diminish in size and then disappear, to tell from the appearance almost the instant that the relay will work. The alloy continues to dissolve and the copper begins to show as a pinkish tint, sometimes very uniform over the whole surface, sometimes much more in some places than in others. After the removal of the alloy, the copper is often perfectly clean and bright, but sometimes is of a dirty color, and a coppery colored powder may be wiped off the wire. If the current is continued, copper will dissolve. At no time during the whole experiment will any gas be given off of the anode, providing the current density is not too great.

TABLE III.—CUBIC CENTIMETERS OF HYDROGEN GAS EVOLVED FROM 10 CM. OF WIRE.

	Current in Amperes.									
	0.1		0.2		0.3		0.4		0.5	
	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.	Tin.	Alloy.
	0.72	0.82	0.68	0.78	0.60	0.73	0.74	0.80	0.60	0.77
	0.72	0.80	0.63	0.74	0.61	0.82	0.72	0.69	0.61	0.82
	0.58	0.74	0.62	0.84	0.63	0.79	0.62	0.84	0.60	0.70
	0.66	0.78	0.66	0.77	0.72	0.82	0.65	0.86	0.70	0.83
Average	0.67	0.79	0.65	0.77	0.64	0.79	0.68	0.80	0.63	0.78

If the current density is too great gas will usually be evolved at the anode. This gas may occur while the tin is bright, or not until some time after the tin and alloy are removed, but usually it begins about the time the free tin is dissolved and the alloy exposed.

The last case is particularly interesting. A suitable current will dissolve first the tin, then the alloy, and then the copper without any evolution of gas. But if the current is increased slightly the tin will dissolve, and the copper would dissolve, but the alloy will not. Some chemical action appears to take place on the surface, for it becomes distinctly lighter in color, insoluble, and apparently a poor conductor of electricity. If the current is continued some time gas will be continuously evolved, and the surface will be found full of little pits where the copper has been dissolved. Apparently the current breaks through this poor conducting film at various places, and it is the very intense current at these spots which evolves the gas. The evolution of gas does not begin gradually but suddenly when the free tin has all been dissolved. The early part of the process, the solution of the tin, is evidently the same regardless of the current density, for experiments have been made beginning with either a strong or a weak current, and changing just before the free tin was all dissolved. The final result is the same in either case that it would have been had a uniform current been used all through. In all cases, wire which has gassed as just described is distinctly lighter in color, either in the solution or after removing it.

Table IV.—Limit of Current Density.

Diameter of Wire, in.	Length Immersed, cm.	Results.	Square Centimeters per Ampere.	Amperes per Square Centimeter.
0.040	greater than 1.6	Good	5.1	0.20
0.040	less " 1.2	Gas	3.8	0.26
0.020	greater than 3.4	Good	5.4	0.18
0.020	less " 2.8	Gas	4.5	0.22
0.010	greater than 9.2	Good	7.4	0.14
0.010	less " 8.4	Gas	6.7	0.15

Table IV shows the limit of current density which could be allowed as determined by experiment. A current of 0.1 ampere was used, and different lengths of different sized wire were immersed in dilute sulfuric acid (1 : 10). "Good" means that the free tin and alloy dissolved in proper manner leaving clean copper wire behind. "Gas" means that after the free tin was dissolved gas was suddenly evolved and the alloy remained on the wire as above described.

The limit to current density depends greatly upon the temperature of the solution. A few degrees warmer and the current may be doubled; a few degrees cooler and the limit will be halved.

With relatively strong acid, 1 : 1 by volume, gas is given off while the tin surface remains bright. Acid 1 : 2 or 1 : 3 by volume, gives off gas with moderate currents and no quantitative results were obtained. Very weak acid, 1 : 40, or still more dilute, also gives off gas with rather small current density. It would not appear that acid much stronger than 1 : 5, or much weaker than 1 : 20, would be suitable. Between these limits fairly good results may be obtained as shown in the section on strength of solution.

TABLE V.—POTENTIAL OF COPPER-TIN ALLOYS.

Atomic Percentage of Copper.	Millivolts Tin – Alloy.	Atomic Percentage of Copper.	Millivolts Tin – Alloy.
10	–4	70	+40
20	+3	73	+39
30	–2	74	+42
40	–4	77	+480
50	+4	78	+475
53	+1	80	+485
57	–3	85	+492
60	+4	90	+475
63	–1	95	+490
65	–3	100	+495
68	+46		

The Potential of the Electrode.—When making this investigation, it was surprising to find that a very small quantity of free tin on the wire made it act almost the same as if it were all tin. I have since found out that A. P. Laurie in 1888 pointed out the same fact. In a paper by him,[1] he says an electrode consisting of two separate metals has approximately the potential of the more positive metal, even if the surface is only about 1/1000 or 1/2000 of the other. In the same article he gives a curve of the potential of the copper-tin alloys in stannous sulfate in acid solution.

More recently N. Pushin,[2] has given us a curve of electromotive force of copper-tin alloys in normal sulfuric acid. The figures are reproduced in Table V.

Results in Detail.—Table VI gives the results in detail obtained from a number of samples of different sized wires and shows how greatly the quantity of tin varies from place to place.

CONCLUSIONS.

The electrolytic test proposed gives a ready means for determining the amount of tin and the amount of tin-copper alloy upon the surface of tinned wire. The amount of unalloyed tin which forms the outer envelope is determinable by this method with considerable accuracy. The amount of tin-copper alloy can usually be determined with fair accuracy. This test gives a method of studying the condition of the tin coating which could be applied as a specification test. It plainly reveals a considerable variation in the thickness of the tin coating. That a considerable variation existed which could not be controlled in the usual process of tinning was already understood.

The electrolytic test does not reveal imperfections in the tin coating as plainly as some of the sulfide tests. It is apparent from the uses to which tinned copper wire is put that bare spots or flaws in the tinning exposing the copper would be the most fatal form of defect which such wire could have. There is considerable difference of opinion as to whether the thickness of the tin coating is important so long as it is perfect. Some consumers' specifications call for a very thin coating of tin. Some of the larger consumers of tinned wire have required testing either by exposure to hydrogen-sulfide gas or by dipping in sulfide solutions designed simply to reveal, by blackening of exposed copper, whether or not the tin coating is perfectly continuous.

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[1] *Transactions*, Chem. Soc., Vol. 53, p. 104 (1888).

[2] *Zeitschrift für Anorganische Chemie.*, Vol. 56, p. 1 (1908).

TABLE VI.—RESULTS OF ELECTROLYTIC TESTS.

Size, in.	Sample No.	Electrolytic.					Analysis, Total Tin.		Difference: Analysis-Electrolytic.
		Cu. Cm.		Milligrams per sq. cm.					
		Tin.	Alloy.	Tin.	Alloy.	Total.	Per cent.	Milligrams per sq. cm.	Milligrams per sq. cm.
0.128	1........	1.14	2.11						
		1.18	2.22						
		1.23	1.97						
		1.30	2.01						
		1.22	2.07						
	Average....	1.21	2.08	0.59	0.49	1.08	0.14	1.02	−0.06
	2........	3.42	2.28						
		3.40	2.28						
		3.41	2.60						
		3.92	2.14						
		3.51	2.20						
	Average....	3.53	2.30	1.71	0.54	2.25	0.29	2.12	−0.13
	3........	1.40	2.07						
		1.43	2.15						
		1.46	2.15						
		1.65	2.08						
		1.37	1.88						
		1.38	1.96						
		1.48	1.89						
		1.50	2.15						
	Average....	1.46	2.04	0.71	0.48	1.19	0.16	1.17	−0.02
0.081	1........	1.54	1.21						
		1.52	1.27						
		1.64	1.27						
		1.53	1.32						
		1.62	1.38						
		1.72	1.39						
		1.58	1.44						
		1.52	1.72						
	Average ...	1.58	1.37	1.21	0.51	1.72	0.37	1.71	−0.01
	2........	0.83	1.23						
		0.85	1.14						
		0.80	1.14						
		0.80	1.35						
		0.78	1.24						
		0.82	1.26						
		0.79	1.40						
		0.82	1.19						
	Average....	0.81	1.24	0.62	0.46	1.08	0.25	1.16	0.08
	3........	1.30	1.26						
		1.30	1.20						
		1.18	1.32						
		1.31	1.35						
	Average....	1.27	1.28	0.97	0.47	1.44	0.25	1.16	−0.28
	4........	0.98	1.12						
		1.04	1.29						
		0.99	1.19						
		0.98	1.31						
	Average....	1.00	1.23	0.76	0.46	1.22	0.31	1.43	0.21

TABLE VI.—RESULTS OF ELECTROLYTIC TESTS (*Continued*).

Size, in.	Sample No.	Electrolytic.					Analysis, Total Tin.		Difference: Analysis-Electrolytic.
		Cu. Cm.		Milligrams per sq. cm.					
		Tin.	Alloy.	Tin.	Alloy.	Total.	Per cent.	Milligrams per sq. cm.	Milligrams per sq. cm.
0.081	5	1.24	1.24						
		1.27	1.20						
		1.36	1.26						
		1.40	1.25						
	Average	1.32	1.24	1.01	0.46	1.47	0.33	1.52	0.05
0.072	1	0.66	1.09						
		0.74	1.20						
		0.78	1.17						
		0.78	1.16						
		0.83	1.30						
		0.83	1.19						
		0.85	1.29						
		0.81	1.40						
		0.76	1.09						
		0.74	1.24						
		0.72	1.13						
		0.67	1.10						
		0.70	1.12						
		0.68	1.17						
		0.71	1.11						
		0.68	1.25						
	Average	0.75	1.19	0.65	0.50	1.15	0.32	1.31	0.16
0.064	1	0.94	0.91						
		0.99	0.93						
		0.88	0.92						
		0.98	0.95						
		1.06	0.97						
		0.98	1.20						
		1.04	0.95						
		1.11	0.94						
	Average	1.00	0.97	0.97	0.45	1.42	0.45	1.64	0.22
	2	0.71	0.88						
		0.65	1.02						
		0.72	1.07						
		0.78	0.92						
		0.68	1.00						
		0.67	0.98						
		0.61	0.97						
		0.62	0.92						
	Average	0.68	0.97	0.66	0.45	1.11	0.29	1.06	−0.05
	3	0.72	0.93						
		0.70	0.94						
		0.82	0.88						
		0.78	0.90						
	Average	0.76	0.91	0.74	0.43	1.17	0.27	0.99	−0.18

TABLE VI.—RESULTS OF ELECTROLYTIC TESTS (*Continued*).

Size, in.	Sample No.	Electrolytic.					Analysis, Total Tin.		Difference: Analysis-Electrolytic.
		Cu. Cm.		Milligrams per sq. cm.					
		Tin.	Alloy.	Tin.	Alloy.	Total.	Per cent.	Milligrams per sq. cm.	Milligrams per sq. cm.
0.064	4........	0.87	0.66						
		0.97	0.78						
		0.86	0.75						
		0.83	0.78						
		0.78	0.71						
		0.78	0.76						
		0.77	0.69						
		0.73	0.66						
		0.87	0.79						
		0.78	0.84						
		0.75	0.84						
		0.75	0.75						
		1.04	0.86						
		1.00	0.78						
		0.92	0.97						
		0.82	0.80						
		0.72	0.73						
		0.83	0.84						
		0.81	0.78						
		0.79	0.76						
		1.02	0.70						
		1.00	0.75						
		0.76	0.88						
		0.70	0.73						
	Average....	0.84	0.77	0.81	0.36	1.17	0.31	1.23	0.06
0.051	1........	0.71	0.68						
		0.64	0.82						
		0.62	0.71						
		0.58	0.73						
		0.63	0.70						
		0.64	0.73						
		0.73	0.61						
		0.67	0.77						
		0.66	0.72						
		0.65	0.79						
	Average....	0.65	0.73	0.79	0.43	1.22	0.41	1.19	−0.03
	2........	1.14	0.84						
		1.09	0.86						
		1.16	0.72						
		1.15	0.72						
		1.12	0.75						
		1.10	0.71						
		1.16	0.71						
		1.11	0.74						
	Average....	1.13	0.76	1.38	0.45	1.83	0.58	1.69	−0.14
	3........	0.46	0.70						
		0.43	0.68						
		0.44	0.59						
		0.43	0.59						
		0.43	0.60						
		0.45	0.61						
		0.46	0.59						
		0.49	0.60						
	Average....	0.45	0.62	0.55	0.36	0.91	0.30	0.87	−0.04

TABLE VI.—RESULTS OF ELECTROLYTIC TESTS (*Continued*).

Size, in.	Sample No.	Electrolytic.					Analysis, Total Tin.		Difference: Analysis-Electrolytic.
		Cu. Cm.		Milligrams per sq. cm.					
		Tin.	Alloy.	Tni.	Alloy.	Total.	Per cent.	Milligrams per sq. cm.	Milligrams per sq. cm.
0.040	1........	0.65	0.48						
		0.64	0.44						
		0.58	0.58						
		0.64	0.50						
		0.66	0.48						
		0.66	0.40						
		0.72	0.51						
		0.70	0.40						
	Average....	0.66	0.47	1.02	0.35	1.37	0.61	1.39	0.02
0.020	1........	0.60	0.31						
		0.58	0.32						
		0.56	0.32						
		0.55	0.33						
		0.58	0.30						
		0.55	0.28						
		0.61	0.28						
		0.59	0.31						
	Average....	0.58	0.31	1.80	0.47	2.27	1.85	2.11	−0.16
0.010[a]	1........	0.08	0.14						
		0.08	0.16						
		0.08	0.18						
		0.08	0.14						
		0.08	0.14						
		0.08	0.15						
		0.08	0.18						
		0.09	0.18						
	Average....	0.08	0.16	0.50	0.48	0.98	1.08	0.62	−0.36

[a] The 0.010-in. wire was too small to be seen satisfactorily, and the results are unreliable. The apparatus is not to be recommended for wire less than 0.020 in. in diameter.

One of the more recent specifications which have appeared requires the alternate dipping of tinned wire in hydrochloric-acid and sodium-sulfide solutions of the strength usually described in the Signal Engineers' specifications through two cycles without showing blackened spots or streaks. In the writer's opinion this test is amply sufficient to discover whether the coating of tin is perfect and will perform its function of protecting the copper from the attack of sulfur in the rubber insulating material. If, however, the quantity of tin, either pure, alloyed or total is required, the method of testing described in this paper can be employed to advantage.

DISCUSSION.

The Chairman. THE CHAIRMAN (MR. J. A. CAPP).—Mr. Grower's paper is before you for discussion. If the Chair may be indulged in a brief remark, I should like to say that in my opinion, this paper is an exceedingly important contribution. Those of us who have to deal with tinned copper wire have been troubled for a long time to find an adequate test of the tin coat. There have been many tests proposed, as Mr. Grower has mentioned. All of them are subject to many errors, mostly of personal equation, and there are none entirely satisfactory. The test which Mr. Grower has proposed is certainly very promising. I, for one, hope that many of those who are engaged in the testing of tinned copper wire will make use of it so that experience may be gained. I know that Committee B-1, which has to do with copper wire, will welcome any contribution in the way of data obtained, and any criticisms of the method or suggestions as they are obtained from the use of the method. As Committee B-1 reported at the last annual meeting, it has been at work on methods of testing the tin coat of tinned copper wire and has not yet arrived at a point where it can make any sort of report. Mr. Grower's work has been largely the outcome of the work which was undertaken by Committee B-1; and as chairman of that committee, I should be very glad to receive any communications from anyone who has obtained data by using this method of test.

Mr. Merica. MR. P. D. MERICA (*presented in written form and read by the Secretary*).—I have been most interested in Mr. Grower's very ingenious method of determining the amount of tin in a tin coating on copper, a determination which, as he says, is attended with some difficulties, owing to the complicated nature of the tin coating and the different electrolytic behavior of its constituents.

Mr. Grower has referred to the layer of coating between the tin (or perhaps, more properly speaking, tin-alloy constituent eutectic) and the copper as being little known and consisting possibly of Cu_3Sn. Some work I have published recently on

Mr. Merica. tinned copper sheet indicates that in this case the "alloy" layer is certainly composed of at least two constituents, the outermost having a copper content of about 40 per cent, and the inner one a copper content of about 62 per cent. These constituents are readily distinguishable microscopically. Measurements of electrolytic potential showed that both of these constituents were slightly negative in many reagents toward copper.

Mr. Bassett. MR. W. H. BASSETT.—It is intended to carry out working tests on tinned wire by the method outlined by Mr. Grower to gain such data as Mr. Capp has asked for. I hope that other members of Committee B-1, and also those who are not members but are willing to turn in data on the subject, will contribute, because it is an important matter.

There is one other bit of work which Mr. Grower has done with his apparatus and method of testing that is very interesting. In the rubber coverings of wire it is of course necessary to vulcanize the covering, and in that way the material is subjected to heat. This raised the question as to what happens to the tin coating on the outside of the alloy, and whether the alloy increased in amount if the material was heated, as in vulcanizing. It was found that at 50° C., even after 27 hours, little or no change had taken place. At 100°, however, a decided change in the amount of alloy could be noted after 9 hours, and after 27 hours, about half the tin that was formerly free tin had changed into tin-copper alloy. At 150° in one hour about the same amount of change had gone on, and in 9 hours, about 85 per cent of the tin on the wire was in the form of alloy. After 27 hours the tin had all gone over to the tin-copper alloy. No investigation has been made of the nature of this alloy, but it is known that some of the tin-copper alloys are brittle; and in some instances tin has peeled from a copper surface on account of the nature of the alloy between it and the copper.

Mr. Campbell. MR. WILLIAM CAMPBELL.—In regard to the alloy formed in tinning, if a sample of wire is examined under say 500 diameters, it is generally found that there are three distinct layers. On the outside we get the copper-tin eutectic with or without crystals of tin, which seems to vary both with the copper contents of the bath—that is to say, the amount the bath has been used—

and to some extent with the temperature. Next there is a thin layer of crystals of the compound, presumably, CuSn; and then generally a thicker layer of the very brittle dull-gray compound Cu_3Sn. The structure is very analogous to that which we find in galvanizing: on the outside of a piece of galvanized wire, we find a layer of pure zinc; beneath that come the crystals of the compound $FeZn_7$; and below that the very brittle compound $FeZn_3$. The amount of each determines the character of the alloy coating. The zinc is penetrated by the crystals of the compound $FeZn_7$, and then below that is the distinct layer of the very brittle compound $FeZn_3$. **Mr. Campbell.**

Recently I did some similar work in dipping copper in molten zinc and found a similar triple series. On the outside there was the solid solution of copper and zinc, running say 2 per cent of copper. Below that occurred white grains of the second solid solution, running anywhere from 14 to 16 per cent of copper. Inside of that was the very hard Gamma constituent running 30 to 40 per cent copper. It seems that the amounts of the various constituents are apparently a function both of the temperature of the bath and the time of immersion.

Mr. Bassett's results in regard to annealing are very interesting, because they confirm what Sir William Roberts-Austin first pointed out and a good many of us have suspected, that these cementation alloys change over and tend towards equilibrium at temperatures very much below the melting point of the most fusible constituent of the whole series.

MR. G. G. GROWER.—I do not know that I have anything to add to the discussion. Of course, this investigation was made without reference to microscopic examination, and I am not prepared to say whether the alloy is uniform or variable, but am inclined to think it is variable. However, when a current is passed into acid from a rich tin alloy, the tin will tend to dissolve, leaving finally this alloy of Cu_3Sn; and after that, I believe it will dissolve as a whole. **Mr. Grower.**

LIGHT VERSUS HEAVY REDUCTIONS IN COLD WORKING BRASS.

By W. Reuben Webster.

SUMMARY.

In a paper delivered at the last annual meeting it was contended that it was more desirable in drawing brass tubes to reduce the thickness of the tubes by many light drafts rather than by a few heavy drafts. This is contrary to established practice and has no basis in the experience of brass makers.

In order to obtain some evidence bearing on the matter, samples of brass were reduced equal amounts by single and multiple reductions of the same total magnitude, and examined by physical tests and microscopic observations. In one set of samples the reductions were effected by cold-rolling, and in another by cold-drawing. No indication was found that the material reduced in a single pass was inferior to that reduced by several passes. Some evidence presented itself leading to a belief that the material reduced by multiple drafts was more unevenly strained than was the case with the single draft. A partial investigation was made of this point but not finished.

As a conclusion it is believed that further evidence in support of the contention first quoted is necessary before it can be accepted.

LIGHT VERSUS HEAVY REDUCTIONS IN COLD WORKING BRASS.

By W. Reuben Webster.

In a paper entitled "An Investigation Leading to Specifications for Brass Condenser Tubes," by Prof. A. E. White,[1] delivered at the last annual meeting of the Society, the statement was made that: "It is likewise more desirable to reduce the thickness of the tube by many light drafts than by a few heavy drafts. Such procedure guarantees more thorough and uniform kneading and interlocking of the tube grains."

In the discussion the writer called attention to the fact that no evidence was presented in support of this statement, and that it was contrary to established practice. The proper degree of reduction is later defined by Professor White as one "which should only be just heavy enough to work the metal all the way through" without defining any method of determining what constitutes that degree. It is quite certain that experienced brass workers could not make such a determination. It is felt by the writer that if there was a difference in results obtained by a series of relatively light passes as compared with one heavy pass effecting the same total reduction, the fact could readily be determined. In order to obtain some data on the subject, the following experiments were performed.

A sample of cartridge metal was rolled down to approximately 0.200 in. thick and very carefully annealed. This sample was then cut into two pieces. Sample No. 1 was rolled to 0.098 in. thick in one pass, while sample No. 2 was rolled to precisely the same thickness in six passes. From each piece three test samples were cut and submitted to physical test with the results shown in Table I. A microscopic examination was also made, as shown by Figs. 1 and 2, the former representing sample No. 1 and the latter No. 2. It is evident from both the physical tests and microscopic examination that the only difference between the material which was given six passes and that which was given but one pass is that the latter has a higher tensile strength and slightly lower elongation.

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 151 (1916).

Table I.—Tests of Rolled Cartridge Metal.

Sample No.	Tensile Strength, lb. per sq. in.	Elongation in 4 in., per cent.
1 (One Pass)	79 130	5.0
	79 760	4.25
	77 770	5.0
Average	78 890	4.75
2 (Six Passes)	77 420	4.75
	77 870	5.25
	78 340	4.75
Average	77 880	4.92

As a further check on this experiment a sample of high brass rod containing a small percentage of lead was drawn to ½ in., carefully annealed and treated precisely as in the above outlined case, except that a reduction of 50 per cent was effected in one sample by one draft through a die, while a similar reduction was effected on the other rod by five successive passes.

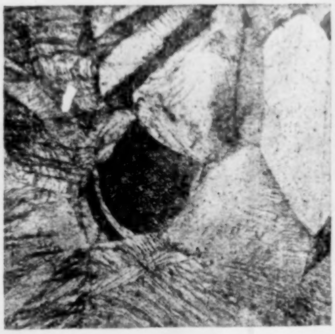


Fig. 1.—Rolled Sheet No. 1 (×75).

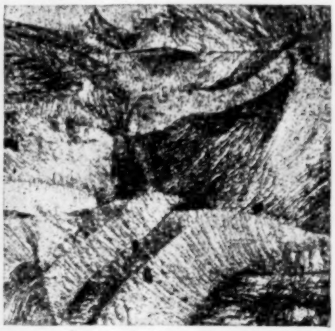


Fig. 2.—Rolled Sheet No. 2 (×75).

The results of physical tests are shown in Table II, while Figs. 3, 4, 5, 6, 7 and 8 show the results of the microscopic examination. Figs. 5 and 8 were taken from the exact center of the two rods and show no difference whatsoever.

It is at least certain that nothing in these observations indicates that any superiority is possessed by the samples from material reduced in several passes, over that reduced in one pass.

Upon examination of the ends of the rods after drawing it was found that those passing last through the die were cupped because of the greater flow of metal near the surface, and that the cup in the rod having five passes was much deeper than in the other case. It is this uneven flow which produces the internal strains which cause season cracking. It was therefore decided to investigate this phenomenon further. A rod of high brass, free from lead, was extruded and drawn to $1\frac{15}{16}$ in. in diameter and carefully annealed. One portion of it was drawn to 1.415 in. in diameter in one pass and an adjacent portion to the same diameter in five passes.

TABLE II.—TESTS OF DRAWN BRASS ROD.

Sample No.	Tensile Strength, lb. per sq. in.	Elongation in 4 in., per cent.
1 (One Pass)	94 450	4.25
	94 300	4.25
Average	94 380	4.25
2 (Five Passes)	93 700	4.25
	93 450	4.25
Average	93 580	4.25

Prior to drawing, the ends of the rods were accurately squared up in the lathe.

Fig. 9 shows the appearance of the ends, the one drawn in one pass having the shallower cup. It is the intention to investigate the relative intensity of the internal strains in these two rods and report the results obtained later. A careful microscopic examination was made of each rod but no differences could be detected.

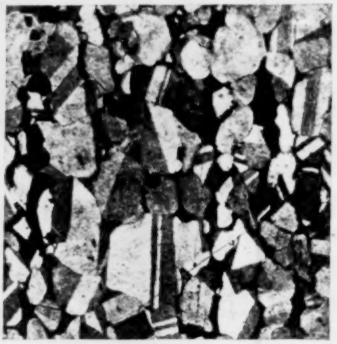


Fig. 3.—½-in. Diameter, Annealed.

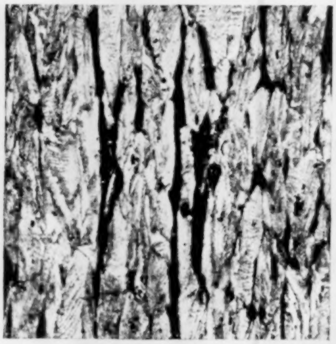


Fig. 4.—0.350-in. Diameter, Edge.

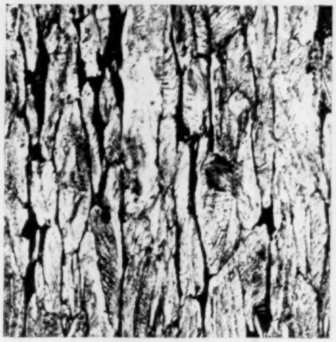


Fig. 5.—0.350-in. Diameter, Center.

Rod No. 1.—One Pass (×75).

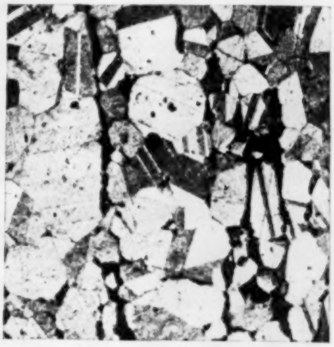


Fig. 6.—½-in. Diameter, Annealed.

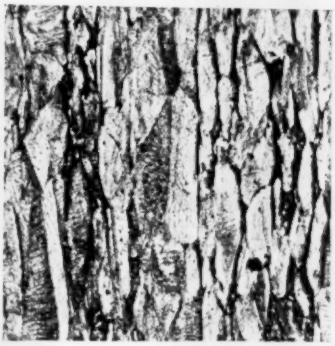


Fig. 7.—0.350-in. Diameter, Edge.

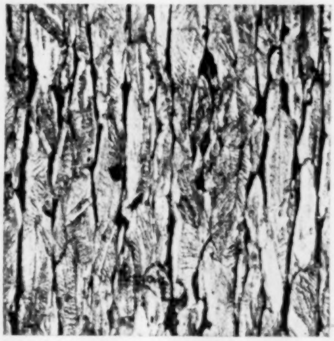
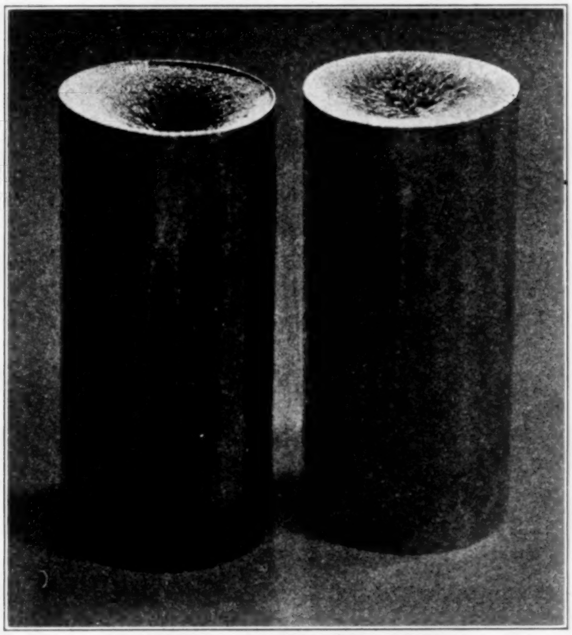


Fig. 8.—0.350-in. Diameter, Center.

Rod No. 2.—Five Passes (×75).

While these experiments cannot be considered as conclusively demonstrating the falsity of the statement quoted at the opening of this paper, at the same time they are better evidence than any evidence that has been presented in support of its truth. It is therefore felt that it is not yet necessary to abandon the existing methods of working brass in favor of that recommended by Professor White.



Five Passes. One Pass.

FIG. 9.

DISCUSSION.

Mr. White. MR. A. E. WHITE.—I am very glad indeed to attempt again to clear up a misapprehension which seems to exist on the part of Mr. Webster concerning my statement of a light draft.

In my paper last year entitled "An Investigation Leading to Specifications for Brass Condenser Tubes,"[1] it was stated that, "It is likewise more desirable to reduce the thickness of the tube by many light drafts than by a few heavy drafts." This was called in question initially by Mr. Price, with the result that an amplification of the term "light draft" was made at that time. This will be found in the discussion of the paper above referred to, and is as follows:

> "It is realized that if the reductions are made with too light a draft there will then result surface or skin hardening. This is an unfortunate condition. It is felt, however, that the draft or reduction which should be given a metal should only be just heavy enough to work the metal all the way through, and that this should be repeated as many times before annealing as the metal will stand, rather than to resort to the use of but one absolute reduction between the annealings."

Too light drafts, resulting only in skin hardenings, is an unfortunate condition. It is this condition which we have just seen illustrated and to which the speaker objects, to as great an extent as does Mr. Webster. It is felt, however, that the draft or reduction which should be given to metal should only be just heavy enough to work the metal all the way through and that this should be repeated as many times before annealing as the metal will stand. I do not know that I can add anything further.

Mr Webster. MR. W. REUBEN WEBSTER.—The question is not one of misapprehension but of measurement. A draft or the amount of a draft is something that is capable of being expressed in measurable terms. It would therefore seem desirable that Mr.

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 151 (1916).

White state specifically what draft is sufficient to work the metal all the way through and what draft is too great. That is something which it is quite confidently stated is not known to the tube drawer. **Mr. Webster.**

MR. HENRY M. HOWE (*by letter*).—The conditions in this case, in which the reduction was made by means of five passes, are so different from those described in the paper by Mr. E. H. Peirce[1] as to suggest that Mr. Webster might well ascertain whether, in his five-pass specimen, there is any appreciable difference between the hardness of the core and of the outside. **Mr. Howe.**

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[1] "Hardness of Hard-Drawn Copper," p. 114.—ED.

TESTING OF SHEET BRASS.

By C. H. Davis.

SUMMARY.

The object of the following investigation is the selection of of a rapid, satisfactory and comparative method of testing sheet brass.

Such methods were employed as are widely used by manufacturers and engineers, namely, tensile strength, scleroscope and Brinell tests. The more recently introduced ductility, or cupping, test was also employed to establish the comparative value of this method of testing. The tests were made upon identical or adjacent pieces of sheet brass, specially gaged and rolled, so that the percentage reduction by rolling is accurately known.

Complete data are given from one of the four brasses tested, to illustrate the methods employed and to show the comparative value and limits of each method of testing. The scleroscope and Brinell tests are found to be unsatisfactory on thin metal. The Brinell test is satisfactory and comparative on thick metal. Scleroscope, Brinell, and tensile strength tests do not vary appreciably with the thickness of the metal. The ductility tests do vary with the thickness of the metal and therefore demonstrate in a practical way the drawing value of metal of any gage, at the same time giving evidence of the grain size and of imperfections in the metal.

Conclusion.—In order to obtain rapid and comparative results in testing sheet brass, both manufacturer and consumer may use the ductility (cupping) test for thin metal, and the Brinell test for thicker metal.

TESTING OF SHEET BRASS.

BY C. H. DAVIS.

The reason for, and the necessity of, testing sheet brass or any other metal, is not merely to ascertain its grain size, hardness number or the number of times it will stand certain impacts: it is to determine in the best possible way whether the metal is suited for the purpose for which it is intended. Experimental data together with practical experience will ultimately determine the fitness of the metal in question for its work. In choosing the direction for the accumulation of this data, uniformity in methods of testing should be agreed upon.

The accepted method or methods of testing should bring out the desired qualities in characters that will be easily and correctly interpreted by manufacturer, engineer, and ultimate consumer. These characters may be hardness numbers, grain-counts, number of impacts, appearance of surface after working, bending, or what not, but in every case the results to be obtained must agree with the results of any observer on any similar instrument or machine. The test must be easily and quickly applied, and the test specimen of a size easily obtained. "The results should be independent of the actual dimensions of the test-piece, or at least related to those dimensions by some simple geometrical law."[1] Any other method of testing as a means of making or fulfilling specifications would be useless.

To determine satisfactory and comparative methods of testing which will satisfy the above conditions, is the purpose of the present investigation.

PREPARATION OF SAMPLES.

Although this work is experimental in nature, yet the material which has been tested to obtain the required data, has in all instances received customary mill treatment. Consequently, the data given represent values for tests upon these particular alloys as commercially prepared. The work has been directed from the Technical Department of the American Brass Co.

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[1] Rosenhain, "Introduction to Physical Metallurgy," p. 194.

Four alloys were cast and prepared in the manner outlined. Certain other material already prepared has been used to illustrate particular points.

TABLE I.—ANALYSIS OF ROLLING OF SPECIMENS.

Stages of the Rolling Process.	Rolled from		Sampled during Rolling at B. & S. Nos. Hard.	B. & S. Gage No. at Nos. 8 Hard.		Treatment of the Portion of Bars Remaining after Sampling.
	B. & S. Gage No.	Thickness, in.		B. & S. Gage No.	Thickness, in.	
PART.						
1..	6	0.1620	......	8	0.1280	These four bars were then annealed, sampled, and rolled as outlined in Part 2 of this Table.
	6	0.1620	......	10	0.1010	
	6	0.1620	......	12	0.0800	
	6	0.1620	......	14	0.0640	
2..	8	0.1280	1, 2, 4, 6, 8	16	0.0500	The remainder of these four bars at 8 B. & S. Nos. Hard was then annealed, sampled, and rolled again as outlined in Part 3 of this Table.
	10	0.1010	1, 2, 4, 6, 8	18	0.0400	
	12	0.0800	1, 2, 4, 6, 8	20	0.0319	
	14	0.0640	1, 2, 4, 6, 8	22	0.0253	
3..	16	0.0500	1, 2, 4, 6, 8	24	0.0201	The remainder of these four bars at 8 B. & S. Nos. Hard was then annealed, sampled, and rolled again as outlined in Part 4 of this Table.
	18	0.0400	1, 2, 4, 6, 8	26	0.0159	
	20	0.0319	1, 2, 4, 6, 8	28	0.0126	
	22	0.0253	1, 2, 4, 6, 8	30	0.0100	
4..	24	0.0201	1, 2, 4, 6[a]			
	26	0.0159	1, 2, 4[a]			
	28	0.0126	1, 2[a]			
	30	0.0100	1			
	30	0.0100	6[b]			

[a] Rolled to 0.0100 in. only. [b] Rolled to 0.0050 in.

The bars were made $1\frac{1}{32}$ in. thick, and were rolled down in the ordinary way to B. & S. gage No. 6 (0.162 in.) and annealed. Table I gives the analysis of the rolling of the specimens.

TABLE II.

Reduction by Rolling, per cent.	B. & S. Numbers Hard.	The B. & S. Gage Number of each Sample Selected for the Tests is given below. (See the left-hand column for the percentage reduction by rolling of these samples).								
00.0	00	14	16	18	20	22	24	26	28	30
10.9[a]	1[a]	15[a]	17[a]	19[a]	21[a]	23[a]	25[a]	27[a]	29[a]	31[a]
20.7	2	14	16	18	20	22	24	26	28	30
37.1	4	14	16	18	20	22	24	26	28	30
50.0	6	14	16	18	20	22	24	26	28	30
60.4	8	14	16	18	20	22	24	26	28	30

[a] Samples were taken on the odd gage numbers to simplify the process of rolling.

In other words, this method of rolling and sampling gave test specimens on B. & S. gages Nos. 14 to 31, inclusive, having the reductions shown in Table II.

Large samples were cut off at each gage and on each temper. From these original samples a central strip was cut for cupping tests 3½ by 12 in., allowing space for three cups. One-inch strips were then cut lengthwise adjacent to the central portion giving duplicate pieces for tensile strength and elongation tests. The scleroscope and Brinell tests were taken on the ends of these 1-in. strips. For the tensile strength test the 1-in. strips were machined into test specimens having a cross-section at the center of 0.5 in. times the thickness of the sample, as shown in Fig. 1.

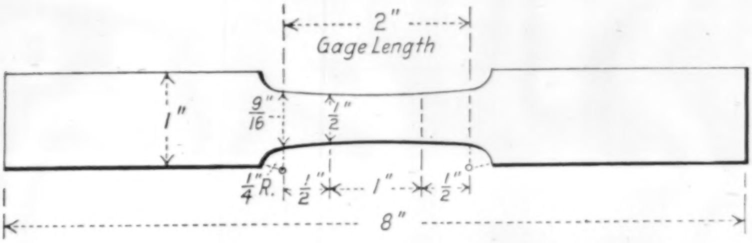

FIG. 1.—Tension Test Specimen.

TESTING MACHINES AND METHODS EMPLOYED.

Tensile Strength Machine.—For tensile strength tests a 10,000-lb. horizontal testing machine was employed. The percentage of elongation in 2 in. and the percentage of reduction of area were calculated from the two tension test specimens.

Scleroscope Testing Machine.—The scleroscopic tests were made with a Shore scleroscope using a steel-pointed magnifier hammer. At least ten tests were made upon each sample.

Ductility Testing Machine.—The cupping tests were made upon an Olsen ductility testing machine. This is a hand testing machine similar in size and action to the Erichsen apparatus for testing thin plates. Since the latter was not obtainable at the time of this research,[1] no comparison can be made between it and the Olsen machine. If the tools and dies of these two machines were made identical, there is no reason to doubt but that the results would be the same from both instruments. The chief difference between the Erichsen and Olsen machines, as far as cupping tests go, is that in the former the tool moves horizontally away from the operator, who must observe the break by means of a mirror; whereas, in the latter, the tool moves upward and allows the operator to carefully watch the breaking of the metal. The sample is held between die and holder, and a tool actuated by a ram and a hand-wheel is moved gradually forward until fracture of the sheet takes place. The depth of indentation is then read off directly from micrometer scales and forms the basis of workability determinations.

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[1] Erichsen tests have been appended to this paper; see p. 194.—ED.

The sheet metal ductility or cupping machines are suitable for testing metal from the thinnest gages up to a gage as thick as the space allowed between die and tool, namely, 0.0625 in. in the Olsen machine. Metal thicker than 0.0625 in. will be sheared between the tool and die except in certain instances where hard-rolled metal breaks before the maximum diameter of the rounded tool reaches the edge of the die. These points are illustrated in Fig. 6. Tests have been successfully made on hard-rolled metal as thick as 0.080 in., whereas annealed brass generally shears between tool and die at about 0.0625 in. in thickness. Tools could be provided for cupping thicker metal, but in that case the hand testing machine would have to be supplanted with a power testing machine. The Erichsen machine has been fully described and illustrated in recent issues of several engineering magazines.

Brinell Ball Impression Test.—The Brinell hardness tests were made with a Swedish apparatus manufactured by the Aktiebolaget Alpha. A load of 500 kg. and the standard 10-mm. diameter ball were used in making the tests. The pressure was maintained for 30 seconds. In this connection it may be stated that experimental data from tests upon both annealed and hard-rolled brass show conclusively that the size of the ball impression is fixed at the instant the maximum pressure, 500 kg., is attained.[1] Samples tested for periods ranging from one second to five minutes yielded impressions of identical size. Therefore, care should be taken to apply the last kilograms slowly, in order that the load may not rise above 500 kg. even for the fraction of a second. The Brinell test may be carried out on any compression testing machine with the addition of a small contrivance.

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[1] A. V. de Forest, "Some Experiments on the Plastic Elongation of Wire," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 455 (1916), and discussion.

The minimum thickness of metal capable of being tested with the standard 10-mm. ball has been given as 2.5 mm. or 0.100 in., but from the data graphically expressed in Fig. 6, it is seen that hard-rolled brass can be safely and correctly tested to about 0.050 in. in thickness, whereas, annealed brass is correctly tested to 0.080 in. and relatively tested to ±0.050 in.

The diameter of the ball impression varies slightly with the direction of rolling in the case of hard metal. This variation is so slight that in all cases where noticeable an average diameter was taken from two readings, one in the direction of rolling and one transverse to the direction of rolling.

TABLE III.—ANALYSES OF COMMERCIAL BRASSES.

Mark.	Copper, per cent.	Zinc, per cent.	Lead, per cent.	Iron, per cent.
A	89.36	10.55	0.07	0.02
B	85.13	14.81	0.04	0.02
C	78.50	21.45	0.02	0.03
D	75.13	24.75	0.10	0.02
E	66.30	33.64	0.02	0.04
F	66.06	33.75	0.16	0.03

RESULTS OF TESTS.

From a mass of data on the results of tests of four commercial brasses whose analyses are given in Table III, a single complete example of the work done on the brass marked E is given in Table IV. Certain other data are also given from the other alloys for purposes of comparison. These data are shown in Table V.

In Table VI are given the results of tests on three brasses (90-10, 80-20 and 66-33 copper-zinc alloys). These are plotted in Fig. 4.

TABLE IV.—RESULTS OF TESTS ON BRASS E.

COPPER, 66.30 PER CENT; ZINC, 33.64 PER CENT; LEAD, 0.02 PER CENT; IRON, 0.04 PER CENT.

Mark (B. & S. Nos. Hard).	Thickness Rolled, in.		Reduction by Rolling, per cent.	Scleroscope Test,[1] Hardness Number.	Brinell Test.[2]		Ductility Test,[3] Depth of Cup, in.	Tensile Strength,[2] lb. per sq. in.	Reduction of Area,[2] per cent.	Elongation in 2 in.,[2] per cent.
	From	To			Diameter of Impression, mm.	Hardness Number.				
Soft	......	0.1580	0.0	....	....	.....	.....	.....	..	....
Soft	......	0.1310	0.0	14.3	3.47	51.2	.....	.....	..	....
2	0.1580	0.1310	17.0	37.7	2.40	109.0	.....	.....	..	....
Soft	......	0.1050	0.0	15.0	3.44	52.2	.....	.....	..	....
4	0.1580	0.1035	34.4	52.0	2.10	143.0	.....	.....	..	....
Soft	......	0.0840	0.0	13.7	3.50	50.3	.....	.....	..	....
6	0.1580	0.0833	47.2	58.5	2.02	155.0	.....	.....	..	....
Soft	......	0.0660	0.0	14.0	3.40	53.4	0.544	47 050	69	57.5
1	0.0660	0.0590	10.6	29.0	2.80	79.5	0.408	51 050	67	43.0
2	0.0840	0.0650	22.6	40.0	2.40	109.0	0.361	59 800	64	22.0
4	0.1050	0.0652	37.9	51.5	2.12	140.0	0.294	73 350	60	8.0
6	0.1310	0.0640	51.1	55.5	2.03	153.0	0.182	83 350	52	6.0
8	0.1580	0.0654	58.6	56.5	2.00	158.0	0.138	90 050	48	5.3
Soft	......	0.0510	0.0	15.0	3.38	54.0	0.532	46 250	69	54.8
1	0.0510	0.0460	9.8	31.0	2.72	84.5	0.390	53 000	62	35.8
2	0.0660	0.0518	21.5	36.5	2.46	103.5	0.351	57 150	59	23.8
4	0.0840	0.0520	38.0	50.0	2.13	139.0	0.281	71 950	42	8.5
6	0.1050	0.0500	52.3	55.0	2.02	155.0	0.192	85 600	34	5.3
8	0.1310	0.0515	60.6	55.5	1.98	161.5	0.152	90 650	41	4.5
Soft	......	0.0406	0.0	15.0	3.31	56.4	0.519	46 200	67	56.5
1	0.0406	0.0353	13.0	34.5	2.70	85.6	0.356	53 450	61	34.5
2	0.0510	0.0422	17.2	36.5	2.53	98.0	0.345	56 600	61	25.8
4	0.0660	0.0405	38.6	51.5	2.11	141.0	0.267	73 300	46	8.3
6	0.0840	0.0410	51.1	56.0	2.02	155.0	0.202	84 050	45	4.3[a]
8	0.1050	0.0417	60.0	54.5	2.00	158.0	0.158	91 700	45	3.0
Soft	......	0.0330	0.0	16.0	3.20	60.5	0.482	46 700	64	50.8
1	0.0330	0.0294	10.9	34.0	2.70	85.7	0.343	52 500	55	34.8
2	0.0406	0.0318	21.6	42.5	2.43	106.0	0.300	59 800	51	18.8
4	0.0510	0.0327	35.8	54.5	2.12	140.0	0.242	75 000	47	7.8
6	0.0660	0.0325	50.7	56.8	2.03	153.0	0.206	84 200	38	4.8
8	0.0840	0.0317	62.2	57.0	1.96	164.0	0.157	91 800	37	2.5
Soft	......	0.0262	0.0	14.0	3.32±	56.1	0.490	45 650	57	51.0
1	0.0262	0.0218	16.7	34.0	2.70	85.7	0.287	54 600	52	27.5
2	0.0330	0.0260	21.2	41.5	2.45	104.0	0.262	59 350	53	20.3
4	0.0406	0.0254	37.4	52.5	2.16	135.0	0.220	73 200	41	6.8
6	0.0510	0.0222	56.4	56.0	1.97	163.0	0.172	91 550	42	2.0
8	0.0660	0.0260	60.6	56.2	1.96	164.0	0.171	91 600	46	2.0
Soft	......	0.0196	0.0	14.7	....	.....	0.458	47 750	55	50.0
1	0.0196	0.0170	13.2	32.0	....	.....	0.286	54 600	48	27.8
2	0.0262	0.0200	23.6	39.0	....	.....	0.259	58 450	44	17.8
4	0.0330	0.0208	36.9	51.0	2.14±	138.0	0.215	73 350	37	6.5
6	0.0406	0.0206	49.2	54.5	2.05±	150.0	0.193	84 850	32	2.3
8	0.0510	0.0205	60.0	56.5	1.97±	163.0	0.163	94 600	34	1.8
Soft	......	0.0158	0.0	15.0	....	.....	0.448	47 750	68	48.8
1	0.0158	0.0140	11.3	31.0	....	.....	0.275	54 750	66	28.5
2	0.0196	0.0160	18.3	36.0	....	.....	0.237	57 550	61	24.5
4	0.0262	0.0160	38.9	49.0	....	.....	0.195	74 950	43	5.3
6	0.0330	0.0163	50.6	51.5	....	.....	0.170	86 400	47	1.8
8	0.0406	0.0162	60.0	56.0	....	.....	0.140	92 250	40	1.5

[1] Average of 10 tests. [2] Average of 2 tests. [3] Average of 3 tests. [a] Metal sheared.

TABLE IV.—RESULTS OF TESTS ON BRASS (*Continued*).

Mark.	Thickness Rolled, in.		Reduction by Rolling, per cent.	Scleroscope Test,[1] Hardness Number.	Brinell Test.[2]		Ductility Test,[3] Depth of Cup, in.	Tensile Strength,[2] lb. per sq. in.	Reduction of Area,[2] per cent.	Elongation in 2 in.,[2] per cent.
	From	To			Diameter of Impression, mm.	Hardness Number.				
Soft	......	0.0126	0.0	14.4	....	.....	0.450	46 000	56	38.8[a]
1	0.0126	0.0112	11.1	28.5	....	.....	0.285	52 750	56	29.3
2	0.0158	0.0124	21.5	40.5	....	.....	0.227	61 100	49	14.5
4	0.0196	0.0126	35.7	47.0	....	.....	0.202	73 500	42	5.8
6	0.0262	0.0128	51.1	48.0	....	.....	0.154	83 650	18	2.0
8	0.0330	0.0124	62.4	52.5	....	.....	0.124	92 750	23	1.3
Soft	......	0.0100	0.0	15.2	....	.....	0.446	43 850	..	28.8[a]
1	0.0100	0.0092	8.0	19.5	....	.....	0.285	51 700	39	30.0
2	0.0126	0.0105	16.6	35.0	....	.....	0.232	56 050	35	20.8
4	0.0158	0.0104	34.1	46.0	....	.....	0.194	72 400	28	6.0
6	0.0196	0.0103	47.4	48.5	....	.....	0.167	84 100	31	2.0
8	0.0262	0.0100	61.8	51.0	....	.....	0.120	91 450	23	1.3
Soft	......	0.0050	0.0	....	....	.....	0.411	.....	..	....
6	0.0100	0.0050	47.0	....	....	.....	0.135	.....	..	....

[1] Average of 10 tests. [2] Average of 2 tests. [3] Average of 3 tests. [a] Metal sheared.

TABLE V.—DUCTILITY, SCLEROSCOPE AND BRINELL HARDNESS TESTS OF ANNEALED SHEET BRASS.

Gage, in.	Temperature at which Annealed $\frac{1}{2}$ hr. and Quenched.														
	450° C.			550° C.			650° C.			750° C.			850° C.		
	Ductility Test, Depth of Cup, in.	Scleroscope Test, Hardness Number.	Brinell Test, Hardness Number.	Ductility Test, Depth of Cup, in.	Scleroscope Test, Hardness Number.	Brinell Test, Hardness Number.	Ductility Test, Depth of Cup, in.	Scleroscope Test, Hardness Number.	Brinell Test, Hardness Number.	Ductility Test, Depth of Cup, in.	Scleroscope Test, Hardness Number.	Brinell Test, Hardness Number.	Ductility Test, Depth of Cup, in.	Scleroscope Test, Hardness Number.	Brinell Test, Hardness Number.
BRASS A.															
0.065	0.484	17.0	60.5	0.519	13.7	55.1	0.531	11.4	50.3	0.540	10.3	48.9	0.540	9.6	46.6
0.0162	0.386	17.0	....	0.406	14	....	0.428	12.0	....	0.445	11.1	....	0.439	10.0	....
0.0100	0.378	21.0	....	0.404	18	....	0.425	14.2	....	0.420	14.0	....	0.403	12.5	....
0.005	0.374	....	....	0.380	....	....	0.394	....	....	0.383	....	....	0.385	....	....
BRASS C.															
0.065	0.522	24.0	75.0	0.535	17.8	64.0	0.577	13.8	53.4	0.587	12.0	50.3	0.588	10.5	46.4
0.0162	0.412	24.0	....	0.451	18.0	....	0.493	14.0	....	0.519	12.1	....	0.510	11.3	....
0.0100	0.402	29.0	....	0.443	21.0	....	0.480	16.6	....	0.480	15.5	....	0.472	13.5	....
0.005	0.386	....	....	0.424	....	....	0.440	....	....	0.440	....	....	0.412	....	....
BRASS E.															
0.065	0.503[a]	25.0	74.5	0.541[a]	17.6	60.5	0.566[a]	13.6	51.8	0.592[a]	11.7	47.5	0.625	11.0	44.9
0.0162	0.380	25.0	....	0.456	17.7	....	0.502	13.8	....	0.511	12.0	....	0.496	11.0	....
0.0100	0.377	27.0	....	0.432	20.0	....	0.470	16.0	....	0.476	15.0	....	0.471	14.5	....
0.005	0.375	....	....	0.411	....	....	0.427	....	....	0.435	....	....	0.430	....	....

[a] Metal sheared at the depth given.

NOTE.—Brass A contained copper, 89.36 per cent; zinc, 10.55 per cent; lead, 0.07 per cent; and iron, 0.02 per cent.
Brass C contained copper, 78.50 per cent; zinc, 21.45 per cent; lead, 0.02 per cent; and iron, 0.03 per cent.
Brass E contained copper, 66.30 per cent; zinc, 33.64 per cent; lead, 0.02 per cent; and iron, 0.04 per cent.

Table VI.—Physical Tests of Brasses.

Specimen No.	Temperature at which Annealed for $\frac{1}{2}$ hr. and Quenched, deg. cent.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.
1...........	450	40 300	37.5
	550	38 600	39.0
	650	37 400	40.0
	750	36 100	39.0
	850	35 300	37.0
2...........	450	49 700	43.0
	550	45 800	48.0
	650	43 500	52.0
	750	42 100	54.0
	850	40 300	57.0
3...........	450	51 400	48.0
	550	47 800	53.0
	650	45 400	59.5
	750	43 300	58.0
	800	41 000	55.0
	850	38 500	51.0

Analysis.

Specimen No.	Copper, per cent.	Zinc, per cent.	Lead, per cent.	Iron, per cent.
1....................	90.47	9.49	0.01	0.03
2....................	80.80	19.14	0.04	0.02
3....................	67.19	32.77	0.02	0.02

Diagrams Illustrating Points of Comparison between Tests.

The important features of the several tests may be emphasized by reference to Figs. 2 to 13, inclusive. The data for these will be found above with references to the corresponding figures. The general description below each figure is intended to be sufficiently clear to allow immediate conclusions to be drawn.

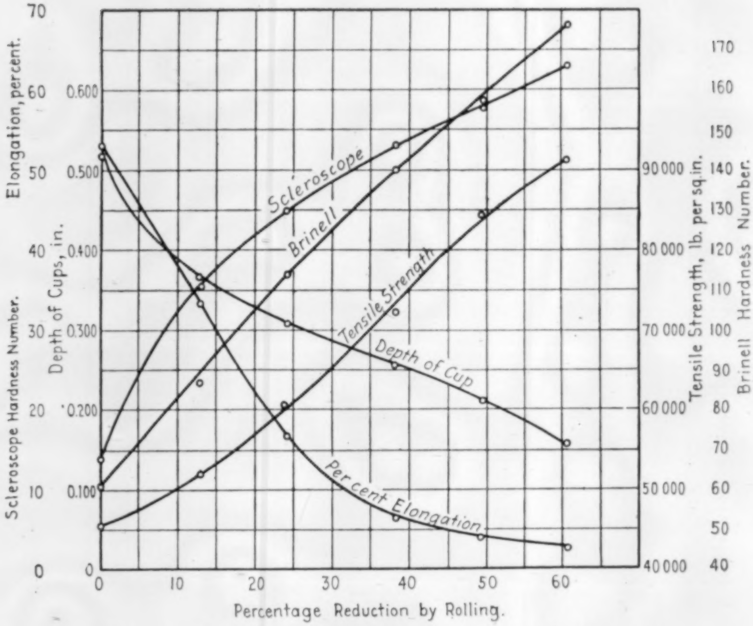

FIG. 2.—Comparison of scleroscope, Brinell, ductility and tensile strength tests on brass containing 66.06 per cent of copper, 33.75 per cent of zinc, 0.16 per cent of lead and 0.03 per cent of iron. Samples were rolled from different gages to 0.040 in. so that the percentage of reduction by rolling increased from 0 to 60 per cent.

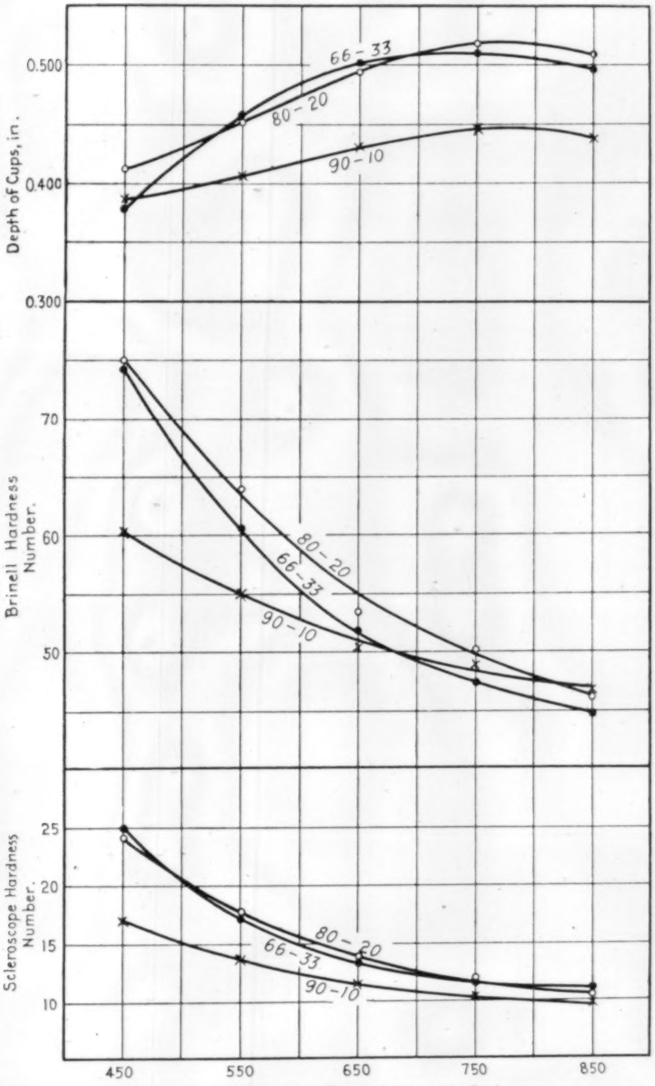

FIG. 3.—Comparison of scleroscope, Brinell and ductility tests on three brasses (90–10; 80–20; and 66–33; copper-zinc alloys). Also, to be compared with tensile strength and elongation tests, Fig. 4. The metal was annealed one-half hour at 450, 550, 650, 750 and 850° C., and quenched. Compare with Plate IV, Fig. B, and Plate II. Data from Table V.

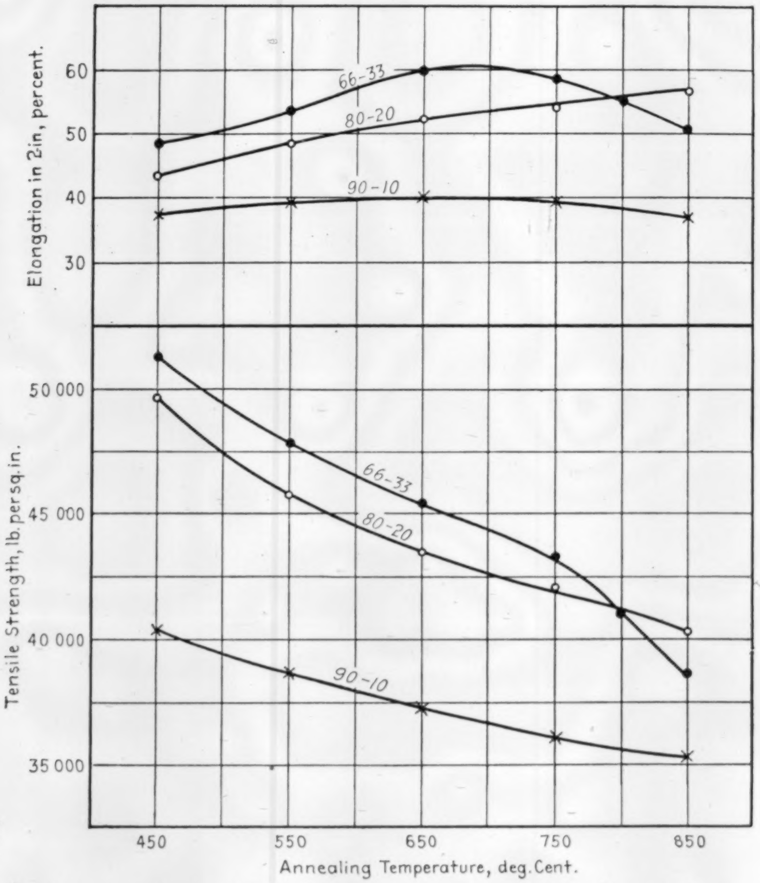

FIG. 4.—Tensile strength and elongation tests on three brasses (90–10; 80–20; 66–33; copper-zinc alloys). The metal was annealed one-half hour at 450, 550, 650, 750 and 850° C., and quenched. Compare with Fig. 3, Plate IV, Fig. B, and Plate II. Data from Table VI.

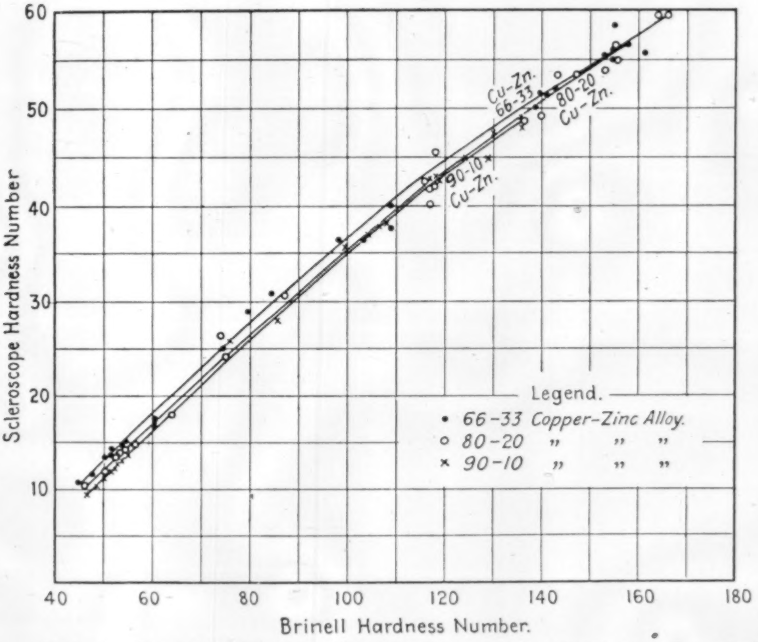

FIG. 5.—The relation between scleroscope and Brinell tests on the same samples is only constant for a definite alloy or metal.[1] This fact supports the contention that the scleroscope does not measure the same "quality" in the metal as does the Brinell test. The data were obtained from tests on three brasses (90-10; 80-20; 66-33; copper-zinc alloys A, C, and E).

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[1] See Devries, "Hardness in its Relation to Other Physical Properties," *Proceedings* Am. Soc. Test. Mats., Vol. XI, p. 726 (1911).

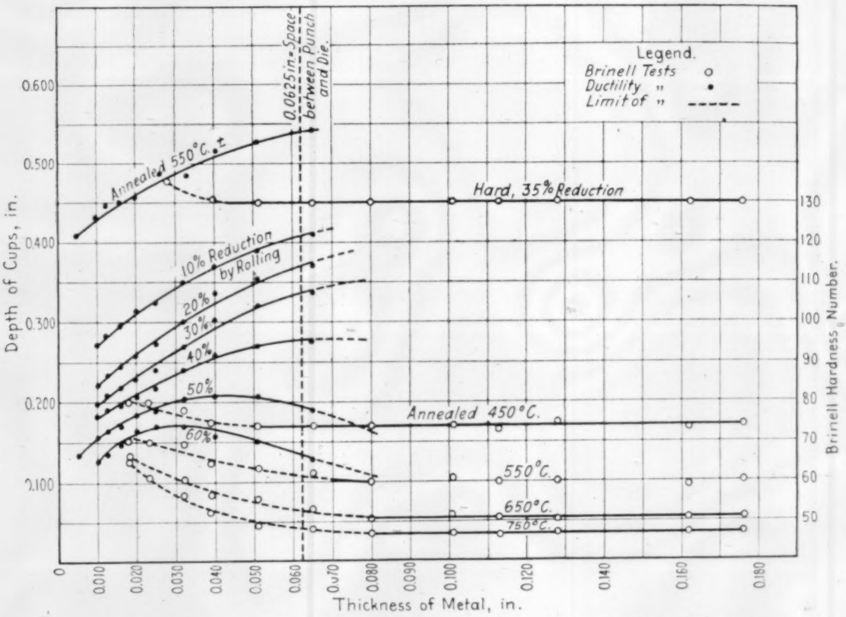

FIG. 6.—A comparison of the successfulness of ductility and Brinell tests on thin metal. Brass E, containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron. Data from cupping tests (Fig. 13) are here superposed on a plot showing where Brinell tests cease to be comparative for both hard-rolled and annealed brass. The region where successful overlapping occurs is ± 0.0625 in.

The Brinell test (10-mm. ball) can be correctly used on brass as thin as 0.050 in. when hard rolled, and from 0.050 to 0.080 in. when annealed.

The ductility or cupping test can be successfully used on all thin metal up to 0.0625 in. (the space between the tool and die).

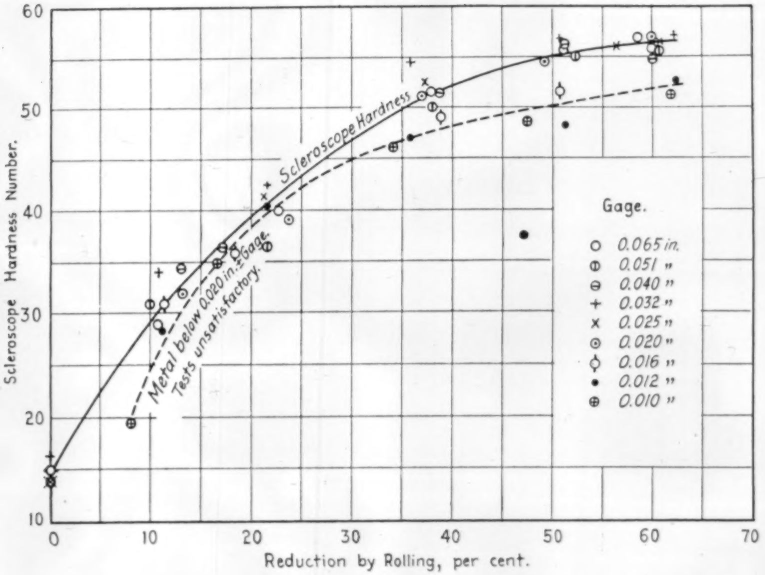

FIG. 7.—The scleroscope hardness tests on annealed and hard-rolled metal do not vary appreciably with the thickness of the metal. Tests on metal below 0.020 in. gage are not comparable with those on thicker metal, and are generally unsatisfactory. Brass E, containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron.

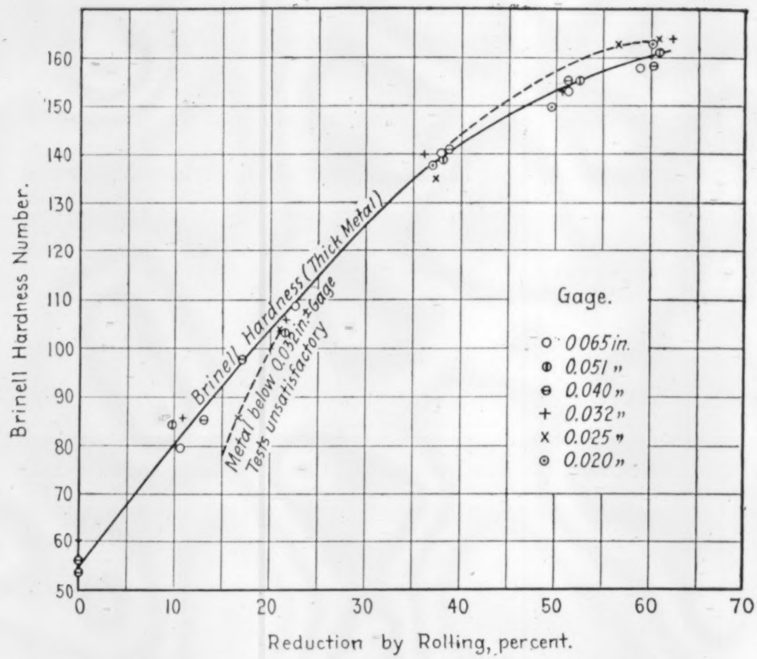

FIG. 8.—The Brinell hardness tests on annealed and hard-rolled metal do not vary appreciably with the thickness of the metal. Tests on annealed metal thinner than 0.050 to 0.080 in., and on hard metal thinner than 0.050 in., are not comparative with tests on thicker metal, and are generally unsatisfactory. (Compare with Fig. 6.) Brass E, containing 66.30 per cent copper 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron.

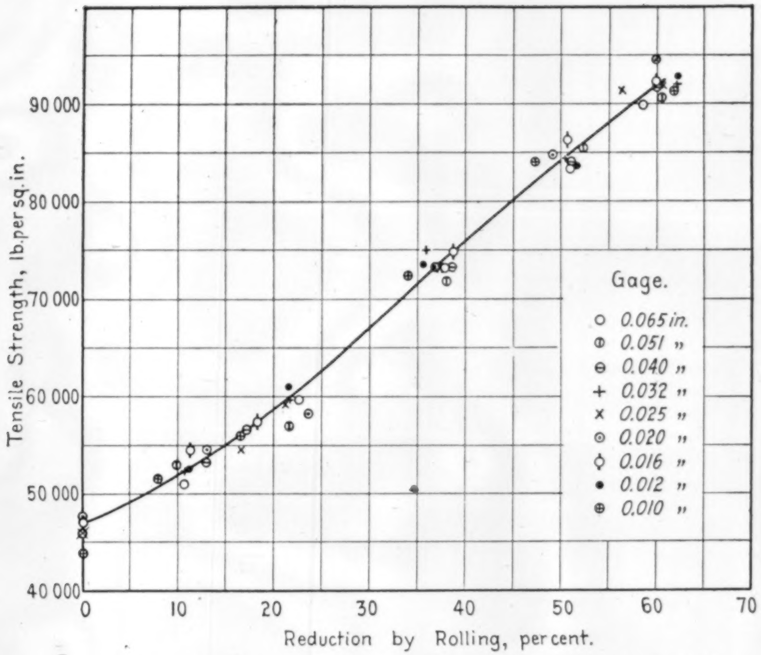

FIG. 9.—Tensile strength tests upon annealed and hard-rolled brass do not vary appreciably with the thickness of the metal. (Compare with Figs. 7 and 8.) Brass E, containing 66.30 per cent copper, 33.64 per cent of zinc, 0.02 per cent lead, and 0.04 per cent of iron.

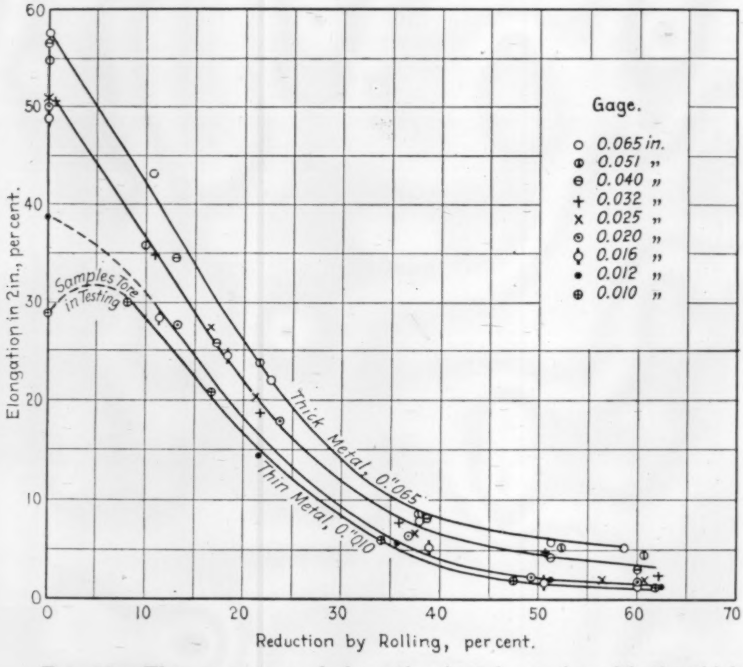

FIG. 10.—The percentage of elongation in 2 in. varies with the thickness of the metal. Compare with the larger variation in the ductility tests shown in Fig. 12. Brass E, containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron.

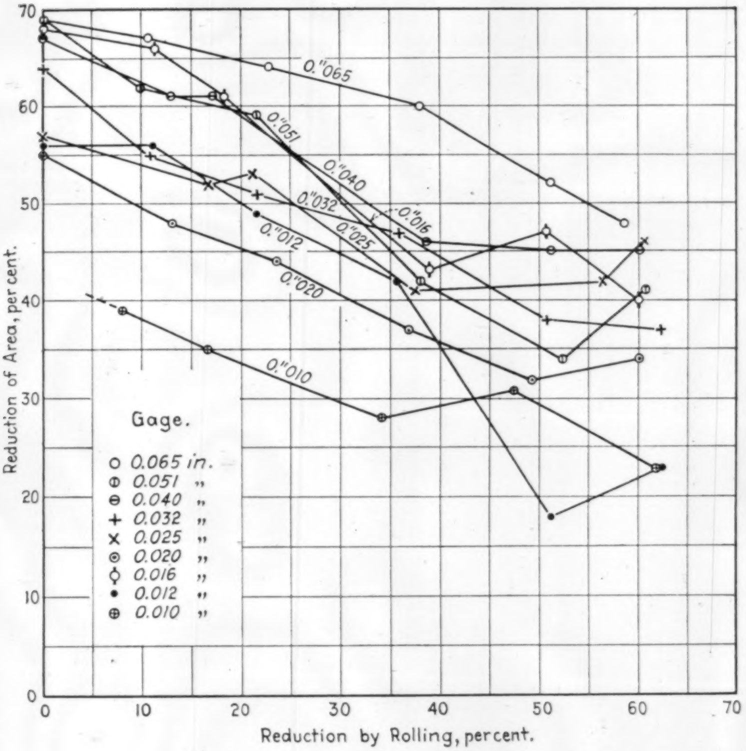

FIG. 11.—The percentage of reduction of area is of little or no value in determining the quality of thin sheet metal. The tests are erratic and not comparable to similar tests on thick metal. Brass E, containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron.

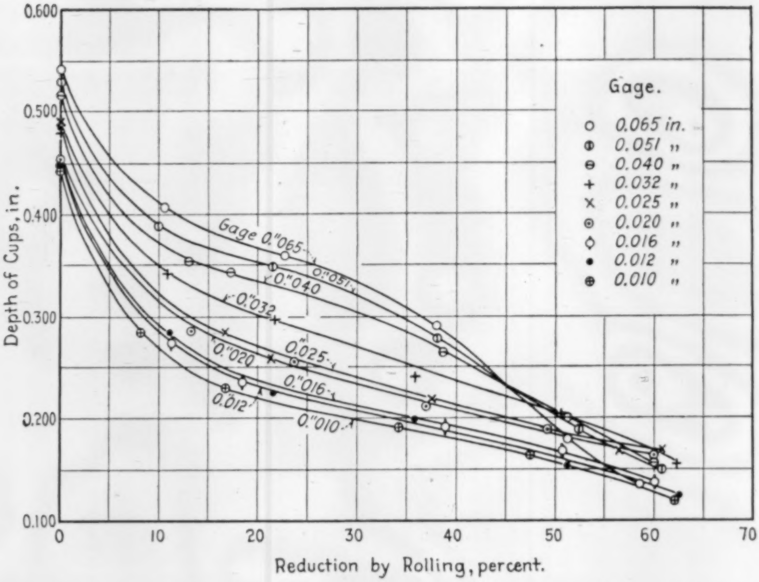

FIG. 12.—The ductility or cupping test can be successfully used on all sheet metal from the thinnest to the limits of the machine, +0.0625 in. Illustration of tests performed on sheet brass (E) containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron. Reduction by rolling, 0 to 60 per cent. Note the change in the drawing value of the metal with differences in gage; also note that the ductility of hard-rolled thick metal (0.065-in. gage) is less than that of hard-rolled thin metal (±0.020-in. gage). Compare with scleroscope, Brinell and physical tests made on the same samples. (Figs. 7 to 11, inclusive.) Compare also with Plate IV and Plate I.

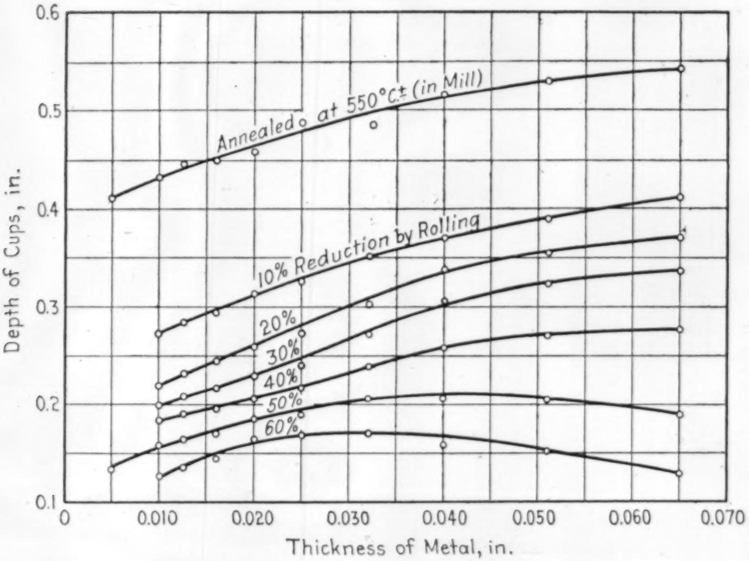

FIG. 13.—Ductility or cupping tests replotted from Fig. 12 to illustrate how accurately the drawing qualities of sheet metal may be estimated for any gage and temper.

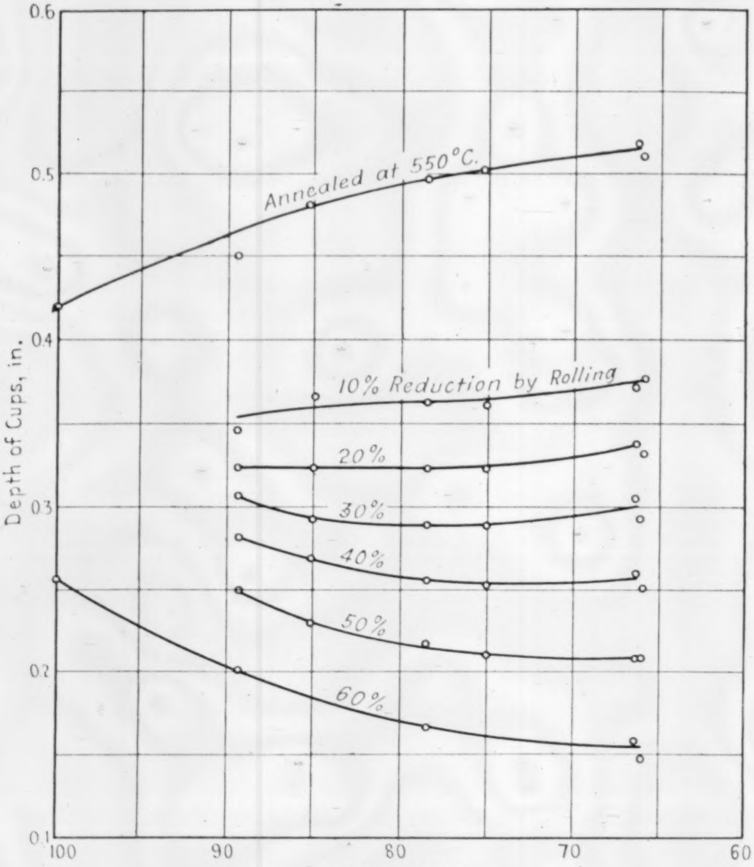

FIG. 14.—Comparative ductility or drawing qualities of six brasses (A to F) 0.040-in. gage, annealed and hard rolled. (See Table III.)

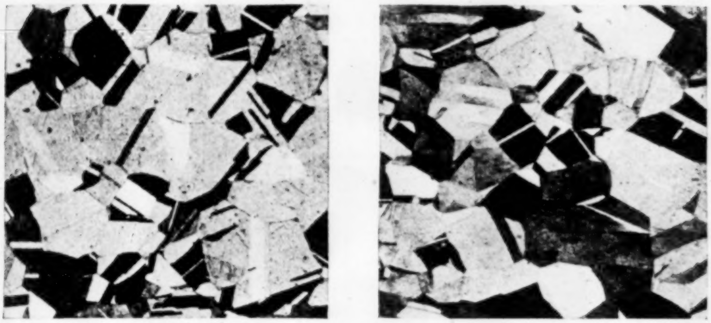

FIG. A.—Annealed at 650° C. FIG. B.—10.6 per cent reduction.

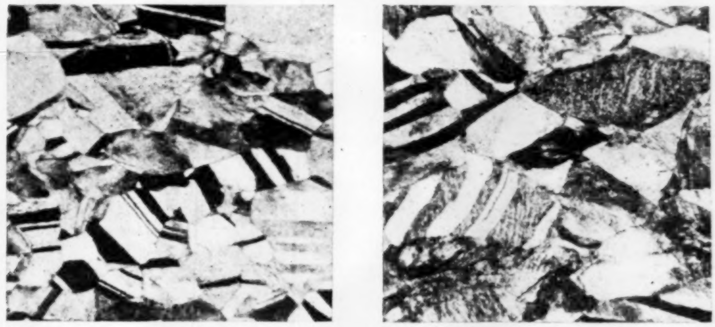

FIG. C.—21.5 per cent reduction. FIG. D.—38.6 per cent reduction.

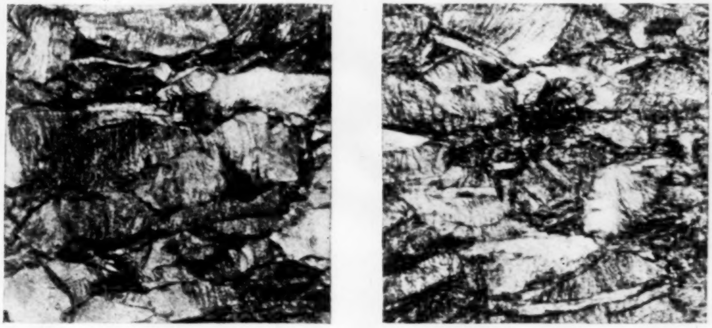

FIG. E.—50.7 per cent reduction. FIG. F.—59.9 per cent reduction.

PLATE I.—Rolling Series. Brass containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron (× 75.) Compare with Figs. 7 to 12, inclusive, and Fig. A, Plate IV.

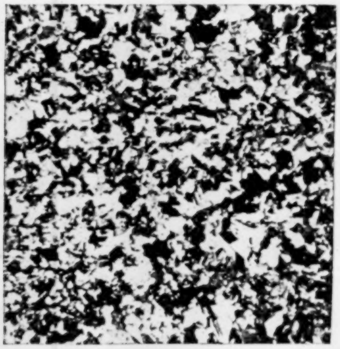

FIG. A.—Annealed at 450° C.

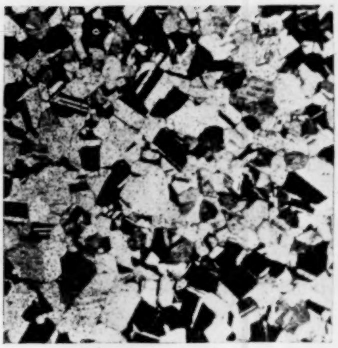

FIG. B.—Annealed at 550° C.

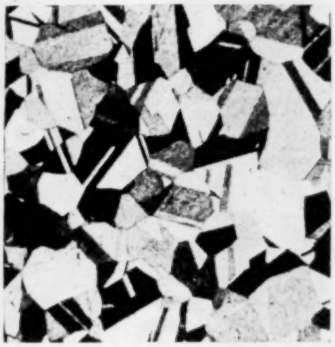

FIG. C.—Annealed at 650° C.

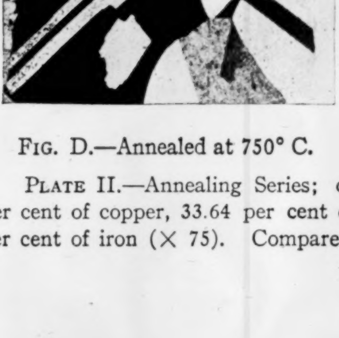

FIG. D.—Annealed at 750° C.

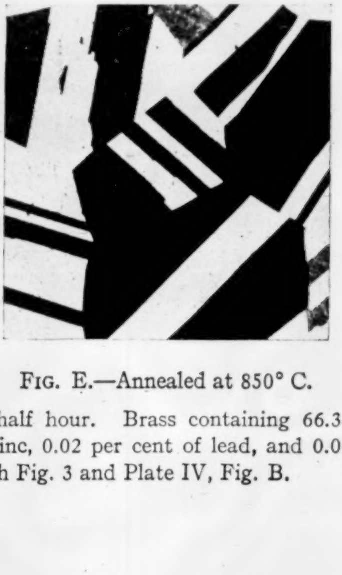

FIG. E.—Annealed at 850° C.

PLATE II.—Annealing Series; one-half hour. Brass containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron (× 75). Compare with Fig. 3 and Plate IV, Fig. B.

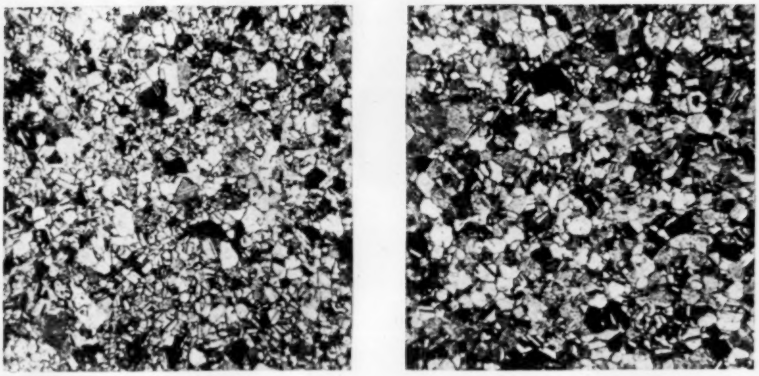

FIG. A.—Brass containing 89.36 per cent of copper, 10.55 per cent of zinc, 0.07 per cent of lead, and 0.02 per cent of iron.

FIG. B.—Brass containing 78.50 per cent of copper, 21.45 per cent of zinc, 0.02 per cent of lead, and 0.03 per cent of iron.

PLATE III.—Brasses annealed one-half hour at 550° C. (× 75). Compare with Plate II, Fig. B.

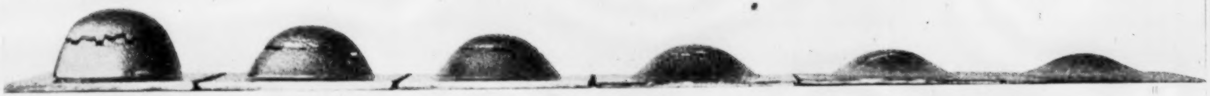
0
10.6
21.5
38.6
50.7
59.9
Reduction by Rolling, per cent.
0.066
0.059
0.0518
0.0405
0.0325
0.0260
Gage, in.

FIG. A.—Compare with Fig. 12 and Plate I.

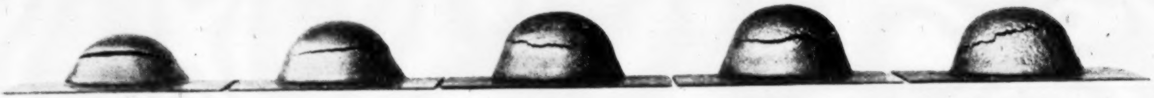
450° C.
550° C.
650° C.
750° C.
850° C.
Annealed ½ hour at Temperatures Noted and Quenched.

FIG. B.—Compare with Fig. 3 and Plate II.

PLATE IV.—Ductility Tests on Brass E, 0.016-in. Gage, containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead and 0.04 per cent of iron.

GENERAL LIMITATIONS OF VARIOUS METHODS OF TESTING FOR THE HARDNESS AND DUCTILITY OF SHEET METAL.

Tensile Strength Tests.—Long experience and conclusively comparative data have established the tension test as a reliable and universal method of determining the character of metal. It is evident that the scleroscope, Brinell or other hardness testing machines will not reveal any quality in metal except its hardness. This is of great value, but hardness cannot be translated into ductility or elongation, nor does it necessarily mean strength. The tension test, however, requires time, both in preparation of the samples and in making the test. Moreover, it is oftentimes impossible to execute the test properly on account of the size and shape of the sample. A skillful and experienced operator is essential, especially in testing thin metal. The requirement of the engineer and consumer is a simple, rapid and comparative method of testing the workability of sheet metal.

Scleroscope Tests.—The Shore scleroscope can be eliminated as a means of *comparative* testing, since from no two instruments are results identical. All instruments could be carefully calibrated, but there still remain unavoidable discrepancies in results due to the following:

1. Operator (personal equation).
2. Different hammers vary in readings from one to seven points on the same instrument.[1]
3. Rapidity of testing changes readings.
4. The observed hardness of thin annealed samples decreases with the thickness. (See Table V.)
5. Observed hardness of hard-rolled samples increases with the thickness of the sample.[2] (See Table V.)
6. The observed hardness is increased by superposing well-fitting specimens. Inaccuracies of fit outweigh the stiffening effect of the increase of mass.[2]
7. A different constant is necessary for recalculating sclero-

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[1] Three magnifier hammers were purchased for this particular instrument.

[2] Howe and Levy, "Notes on the Hardening and Tempering of Eutectoid Carbon Steel nd on the Shore Test," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 5 (1916).

scope hardness into terms of other physical properties for every alloy tested.[1, 2]

8. Scleroscope hardness does not determine grain size.[3]

The great value of the scleroscope lies in the rapidity of the test; in concordant results with one instrument and one operator; in the possibility of testing very small samples and the edges of specimens; in the exploration of small areas which may vary in hardness; and the portability of instrument.

Grain Size.—Measurement or estimation of the grain size of brass as an indication of hardness is practical only with annealed metal, and even in this case the following difficulties are encountered which eliminate this test as a comparative method of obtaining the hardness of brass:

1. The grain size varies rapidly with the percentage of copper.

2. The grain size varies appreciably with the presence of certain impurities, such as iron in brass, arsenic and nickel in copper and rich copper alloys.

3. The grain size varies with the condition of the surface. Under ordinary annealing conditions the grains near the surface are smaller than those in the interior of the bar. In thin metal, which cannot be filed, the true annealing temperature may be misjudged.

4. (*a*) The grain size depends upon the amount and nature of previous workings.[3]

(*b*) Examination of grain size does not show working amounting to less than 10 per cent reduction by rolling.

5. The grain size depends upon the temperature of the annealing previous to the last anneal, even when the metal has been reduced 40 or 50 per cent by rolling between these anneals.

6. A properly etched sample requires considerable time and skill in its preparation. If the sample is not well prepared by deep grinding, smaller surface grains are counted. If the sample is etched roughly with strong acids, the correct boundaries of

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[1] Rose, *Journal*, Institute of Metals, No. 2, Vol. VIII, 1912, pp. 86–125; Mathewson and Phillips, "Recrystallization of Cold-Worked Alpha Brass," *Transactions*, Am. Inst. Min. Engrs., Vol. LIV, p. 608 (1916); Devries, "A Comparison of Five Methods of Hardness Measurement," *Proceedings*, Am. Soc. Test. Mats., Vol. XI, p. 709 (1911), and "Hardness in Its Relation to Other Physical Properties," *ibid*, Vol. XI, p. 726 (1911).

[2] This statement may apply also to the Brinell test.

[3] Mathewson and Phillips, *loc. cit.*

the large grains are obscured while the small grains are often lost entirely in counting.

7. Finally, the personal equation destroys what value this rapid method of grain count has; for instance, a certain laboratory submitted 23 samples giving their grain sizes as 0.050 to 0.065 mm. The same samples carefully prepared gave grain sizes ranging from 0.050 to 0.115 mm.

Since the estimation of hardness by grain size can only be applied to annealed metal, and since there are chances of error even in carefully prepared samples, grain size as a test for hardness merely, should not be considered a comparative method for testing sheet brass.

Brinell Hardness.—The Brinell method offers a means of testing wherein the personal equation is entirely eliminated. The only conditional factor is that the metal be of sufficient thickness to receive the impression of the ball without limiting the size of that impression.

The diameter of the steel ball is constant, the force exerted is constant, and the reading of the diameter of the ball impression is made by means of a fixed scale in the reading microscope.

The Brinell testing method is rapid, simple, gives constant and comparable results with every machine and every operator, and has a percentage of error less than that of the scleroscope or of rapid methods of grain count. The Brinell test is satisfactory on all gages above 0.050 to 0.080 in.—depending upon the hardness of the metal. Therefore, it can be recommended for use in making and checking specifications for the annealing and rolling of brass.

Ductility or Cupping Machine.—The Olsen ductility machine or any similar machine is of extreme practical value in determining the drawing value of thin sheet metal up to about 0.0625 in. in thickness.

Not only can annealed metal be tested to meet drawing-and-cupping specifications, but hard-rolled metal as well has a drawing value relatively proportionate to its percentage reduction by rolling. These two points are illustrated graphically in Figs. 12 and 13. Fig. 14 gives the relative ductility (drawing quality) of six brasses, showing what may be expected of almost any copper-zinc alloy at the thickness tested, 0.040 in.

The following points are worthy of notice:

1. The depth of draw of annealed brass increases with the thickness of the metal.

2. The depth of draw of annealed brass increases with the temperature of annealing until the metal is overheated or "burned." The decrease in ductility at this point corresponds to the decrease in tensile strength and elongation.

3. The depth of draw of hard-rolled brass is relatively proportionate to the percentage reduction by rolling.

4. By observing the smoothness or roughness of the outer surface of the cup, an indication of the heat treatment and grain size of the metal is obtained. The character of the break itself will indicate not only grain size, but will reveal the presence of any small cracks and other imperfections in the metal.

APPENDIX.

THE ERICHSEN MACHINE FOR TESTING METAL SHEETS.

Since the presentation of the original paper on "Testing of Sheet Brass," it has become possible to obtain the Erichsen apparatus for testing sheet metal. Therefore, the metal prepared for the tests of Table IV has been tested upon the Erichsen machine. The results of these tests are given in Table VII, and are directly comparable with the results of the tests on the Olsen machine in Table IV.

TABLE VII.—RESULTS OF ERICHSEN TESTS ON BRASS.

COPPER, 66.30 PER CENT; ZINC, 33.64 PER CENT; LEAD, 0.02 PER CENT; IRON, 0.04 PER CENT.

Mark, B. & S. Nos. Hard.	Thickness Rolled, in.		Reduction by Rolling, per cent.	Depth of Impression.	
	From	To		mm.	in.
Soft	......	0.0655	0.0	14.16	0.557
1	0.0655	0.059	10.0	11.42	0.449
2	0.084	0.065	22.6	9.68	0.381
4	0.105	0.0652	37.9	7.72	0.304
6	0.131	0.0645	49.9	4.53	0.178
8	0.158	0.065	58.9	3.18	0.125
Soft	......	0.051	0.0	13.85	0.545
1	0.051	0.046	9.8	10.58	0.417
2	0.0655	0.052	20.6	9.56	0.376
4	0.084	0.052	38.0	7.28	0.286
6	0.105	0.0505	52.3	5.03	0.198
8	0.131	0.0512	60.6	3.27	0.129
Soft	......	0.041	0.0	13.43	0.529
1	0.041	0.0354	13.6	9.73	0.383
2	0.051	0.0424	16.8	9.21	0.362
4	0.0655	0.0404	38.3	6.97	0.274
6	0.084	0.0413	51.1	5.35	0.210
8	0.105	0.0395	62.3	3.46	0.136
Soft	......	0.0337	0.0	13.19	0.519
1	0.0337	0.0298	11.5	9.80	0.385
2	0.041	0.0320	21.9	7.92	0.312
4	0.051	0.033	35.3	7.13	0.281
6	0.0655	0.0323	50.7	5.50	0.216
8	0.084	0.0322	61.7	3.79	0.149
Soft	......	0.0261	0.0	13.16	0.518
1	0.0261	0.0222	14.8	8.37	0.329
2	0.0337	0.0266	21.0	7.88	0.310
4	0.041	0.0260	36.6	6.19	0.243
6 (7)	0.051	0.0225	55.8	4.37	0.172
8	0.0655	0.0267	59.2	3.95	0.155
Soft	......	0.0195	0.0	12.81	0.505
1	0.0195	0.0177	9.2	8.52	0.335
2	0.0261	0.0203	22.1	6.97	0.274
4	0.0337	0.0216	35.8	6.32	0.249
6	0.041	0.0207	49.5	4.75	0.187
8	0.051	0.0205	59.8	3.74	0.147
Soft	......	0.0164	0.0	12.75	0.502
1	0.0164	0.0142	13.3	8.07	0.317
2	0.0195	0.0164	15.8	7.58	0.298
4	0.0261	0.0164	37.1	5.33	0.210
6	0.0337	0.0165	51.0	4.40	0.173
8	0.041	0.0167	59.3	3.59	0.141
Soft	......	0.0124	0.0	12.80	0.504
1	0.0124	0.0115	7.2	8.02	0.315
2	0.0164	0.0127	22.5	6.20	0.244
4	0.0195	0.0131	32.8	5.55	0.218
6	0.0261	0.0135	48.2	4.25	0.167
8	0.0337	0.0130	61.4	3.57	0.140
Soft	......	0.010	....	12.28	0.483
1	0.010	0.0093	7.0	8.18	0.322
2	0.0124	0.0108	12.8	6.66	0.262
4	0.0164	0.0108	34.1	5.63	0.222
6	0.0195	0.0106	45.6	4.09	0.161
8	0.0261	0.0103	60.5	3.09	0.122

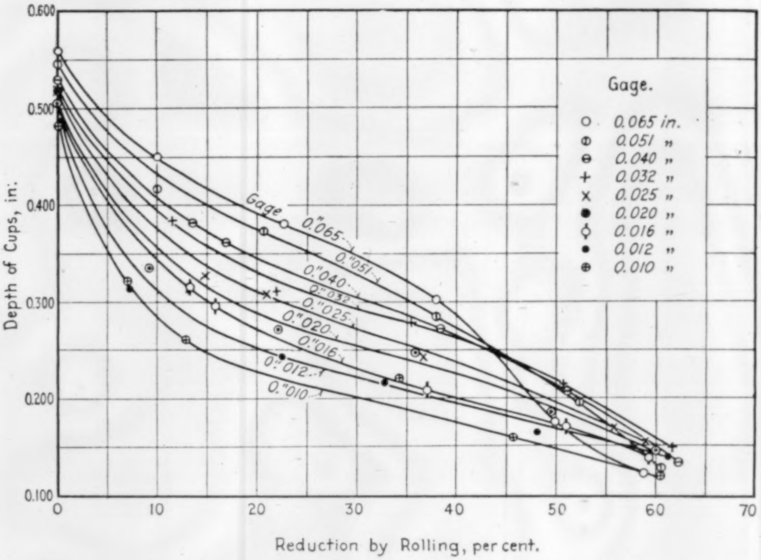

Fig. 15.—Cupping tests with the Erichsen machine to compare with those from the Olsen machine. Brass containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron. Compare with Fig. 12.

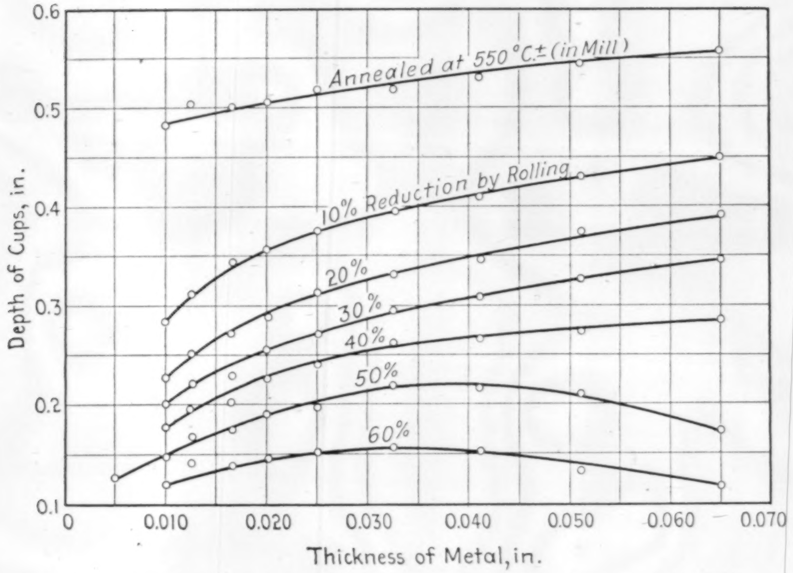

FIG. 16.—Cupping tests with the Erichsen machine replotted from Fig. 15. Brass containing 66.30 per cent of copper, 33.64 per cent of zinc, 0.02 per cent of lead, and 0.04 per cent of iron. Compare with Fig. 13.

COMPARISON.

1. The Erichsen machine registers in millimeters instead of inches.

2. The tool of the Erichsen machine is shaped so that brass up to 0.1 in. in thickness can be successfully tested. The Olsen machine shears metal at about 0.0625 in. in thickness.

3. The depth of the Erichsen impression is somewhat greater with annealed metal and slightly less with hard-rolled metal than that of the Olsen machine. This is due to the smaller end radius and sloping sides of the Erichsen tool.

4. The percentage of variation in three readings on the Olsen and Erichsen machines is about the same. On evenly annealed brass the difference between readings for a number of tests on the same strip may be less than one per cent of the depth of the impression, and would not average more than 1½ per cent. The shallowness of the impression in hard-rolled metal causes an increase in the percentage of variation as high as 6 or 7 per cent.

STANDARD TOOLS.

The ductility or sheet metal testing machines should have a standard tool and die; otherwise, the results from machines of different types will not be comparative. The rapid spread of this method of testing would make the early selection of a standard tool most desirable.

DISCUSSION.

Mr. Holz.

MR. HERMAN A. HOLZ (*presented in written form and read by the author*).—I have studied with considerable interest Mr. Davis' paper recommending the ductility or "cupping" test for sheet brass. The work he has done to define the usefulness and limitations of the various methods of test usually applied to such material is very valuable, especially since results of systematic research with the cupping test have not been published so far. There are now over four hundred Erichsen machines in use in Europe, and nearly one hundred of the original Erichsen machines in this country, but my thorough search in the scientific literature failed to reveal any publication of systematic laboratory tests in reference to the cupping test.

The method of determining the ductility of metal sheets and strips consists in loosely, yet firmly, holding the material between supports during the testing operation, so that the metal is allowed to flow evenly in the direction of a central pressure exerted upon it, gradually applying central pressure thereto until fracture of the metal is obtained, and measuring directly the distance of travel of the central pressing means. This method, which was invented by Mr. A. M. Erichsen, a Norwegian metallurgical engineer, is of utmost importance to the industry. Patents have been granted to him on it in many countries.

There is one point in the application of the Erichsen method which is very important and which I want to emphasize: the sheet to be tested should not be clamped tightly, but must be allowed to flow. It is only in the Erichsen machine that the relative position of the sheet specimen and of the surface holding the specimen can be reliably and accurately adjusted (to 0.01 mm.), and Mr. Erichsen recommends that the distance between the sheet and the holding surface should be 0.05 mm. uniformly on all gages of sheet. Whenever the conditions under which the metal is allowed to flow vary only slightly, the results of tests will be unreliable and erratic; I have been able to ascer-

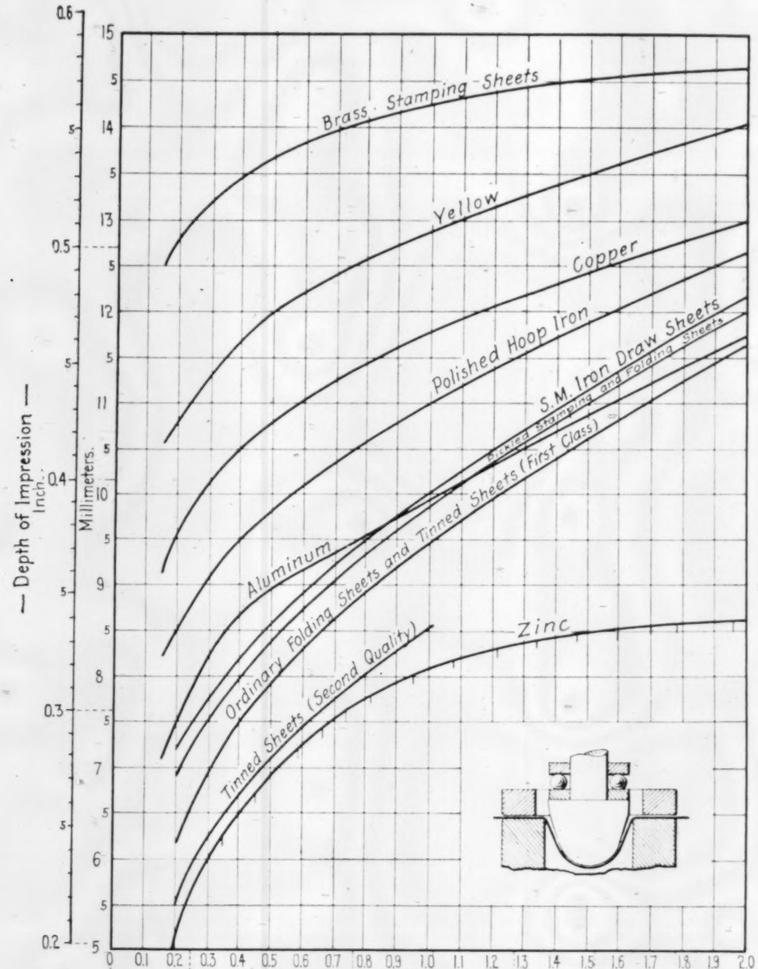
Brass Stamping Sheets
Yellow
Copper
Polished Hoop Iron
S.M. Iron Draw Sheets
Pickled Stamping and Folding Sheets
Aluminum
Ordinary Folding Sheets and Tinned Sheets (First Class)
Tinned Sheets (Second Quality)
Zinc
— Depth of Impression —
Inch.
Millimeters.
0.6
0.5
0.4
0.3
0.2
15
14
13
12
11
10
9
8
7
6
5
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
1.1
1.2
1.3
1.4
1.5
1.6
1.7
1.8
1.9
2.0
Millimeters.
0.01
0.02
0.03
0.04
0.05
0.06
0.07
0.08
Inch.
— Thickness of Sheet —

FIG. 1.

tain this in a large number of tests in my laboratory, and it is quite clear theoretically why this should be so. **Mr. Holz.**

The observation of the cup by means of the mirror in the Erichsen machine is very practical, as the mirror is strongly magnifying; the operator of the machine thus observes the cup practically under a magnifying glass, so that the "end-point" of the test can be established with increased accuracy. The original Erichsen machines are now supplied with gearing for convenient work on heavy sheets (up to $\frac{1}{4}$ in. in thickness), and also with scales divided in inch measurements. The tools and dies in the original Erichsen machines are of such proportions that sheets of the maximum thickness mentioned above will not break or be sheared, but will be drawn to the point of fracture, in accordance with the Erichsen method.

Mr. Erichsen has worked out a number of "standard curves" showing the ratio of minimum draw values of standard qualities of metal sheets, if tested in his machine, and the thickness of the sheets. These curves are submitted herewith (see Fig. 1), and I am sure that they will be of interest. The question of establishing standard specifications for drawing qualities of brass and other metal sheets, differing with the purpose to which the material shall be applied, should be taken up by the Society, because such specifications would considerably facilitate the dealings between producers and purchasers of metal sheets.

In comparing, in general, the various methods available for testing brass sheets, it seems to me that one of the most important advantages of the cupping test lies in the fact that it is a "direct" test for the drawing qualities of the sheet, while tensile and hardness tests are only "indirect" tests. If a manufacturer of metal products purchases metal sheets to be used in drawing operations, he wishes to determine directly the "drawing qualities" of the material delivered, instead of guessing at these properties from information obtained on other properties which may often be misleading.

Mr. Davis' statements in reference to the two methods of determining hardness that are mostly used in practical work are very interesting. I fully agree with him that the Brinell method is the best method now known for such tests, but even this method has serious disadvantages. The Brinell method does

Mr. Holz. not measure the resistance of metal to permanent deformation (that is what we call "hardness"), but it measures resistance of already deformed metal to further deformation. This factor of resistance to further deformation varies considerably in all metals and alloys, so that a Brinell hardness of, say, 150 means something different on brass and on steel, and even on brasses of different composition. This serious disadvantage appears in all methods so far known for determining the hardness of metals. I hope to submit at the next annual meeting of the Society a new method for these investigations which is based on the actual resistance of the metal to permanent deformation and not of deformed metal to various stages of increased deformation. I believe that the value of improved methods of hardness measurement will be more important in the non-ferrous field because in the investigation of iron and steel the method of "magnetic analysis" will give us more information regarding the mechanical properties of the material than any hardness tests.

There is just one more point I desire to mention. The Brinell method, formerly limited to rather small specimens, has been recently considerably extended through the application of the principle of action and reaction. By placing a 10-mm. ball between a material of unknown and a piece of known (predetermined) Brinell hardness, applying a force to the assemblage of the three, and comparing the indentations produced in both materials, the Brinell hardness of any chosen spot of a large sheet can be conveniently and accurately determined. The proportion of Brinell hardness of the two materials varies directly as the spherical areas (squares of diameters) of the two impressions. This method of comparative Brinell tests and a very handy and practical apparatus for its application have been devised and patented by Mr. F. H. Schoenfuss, a member of this Society, and are being used in many hundreds of plants in all parts of the world.

Mr. Davis. MR. C. H. DAVIS (*Author's closure by letter*).—We are indebted to Mr. Holz for his information regarding the Erichsen machine, and especially for Mr. Erichsen's "Standard Curves." The addition of these general data assembles much of our present knowledge of the ductility test within a few pages.

Mr. Davis.

It would be out of place to discuss here the relative values of the two or more cupping machines now on the American market. The mechanism which drives the tool, or which gages the metal, may be very different in any two instruments and still give perfectly satisfactory results. The first thing to do is to determine upon a standard tool and die. It has been shown that the tools of the Erichsen and Olsen machines individually give satisfactory results, but results which are not comparative one with another. It is also evident that the Erichsen tool and die have a wider range of usefulness than those of the Olsen machine. The Society, therefore, should consider the matter of standardizing this most useful test and suggest a definite size and shape of tool and die. Such a specification would be analogous to recommending the employment of a 10-mm. ball for the Brinell test, or the acceptance of a definite length for tension test specimens.

The postponement of the selection of a standard tool will increase the confusion which has already arisen through the lack of cooperation among designers of sheet-metal testing machines. Standard specifications for drawing qualities of metal sheets cannot be made until there is complete agreement regarding the form and size of the tool and die.

INTERIOR SURFACE DEFECTS ON BRASS CONDENSER TUBES AS A CAUSE OF CORROSION.

By W. Reuben Webster.

SUMMARY.

It is the belief of some engineers that defects on the interior surfaces of brass condenser tubes act to accelerate corrosion and that accordingly their presence even to a small degree should not be tolerated.

Extended observation has failed to furnish a basis for such a belief. Many observations have developed the fact that that variety of corrosion which exhibits itself in local pitting resulting in perforation, takes place independently of any interior defects which may exist. No tendency of the pitting to localize on or penetrate the tube at a surface defect has been observed.

Reproductions of photographs of the interior surfaces of corroded tubes, both with and without defects of the character in question, are shown, from which it is clearly seen that corrosion has taken place independently of the defects.

It is not intended to argue that defects of this nature are desirable, but their visibility is decreased by methods of manufacture otherwise undesirable, and increased by treatment conducive to the greatest degree of endurance to corrosion.

INTERIOR SURFACE DEFECTS ON BRASS CONDENSER TUBES AS A CAUSE OF CORROSION.

By W. Reuben Webster.

It is a common experience to find clauses in specifications which have been adopted by the writer thereof because the requirements which they demand appear to be reasonable but which have, as a matter of fact, no basis either in theory or experience.

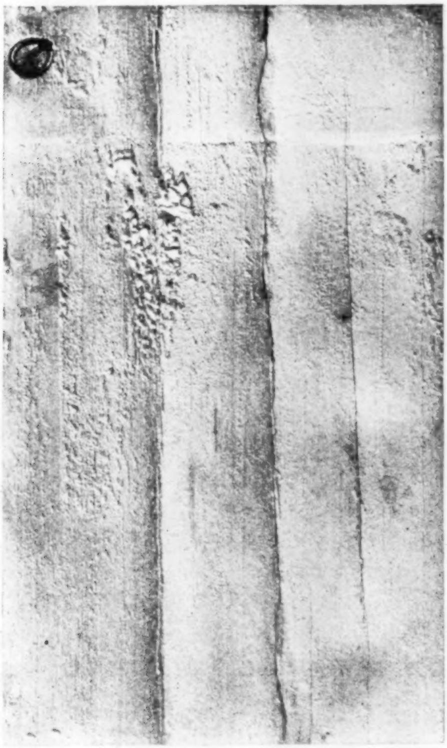

Fig. 1.—Corrosion on Tube Containing Interior Surface Defects.

Certain users of brass condenser tubes have been impressed with the belief that interior surface defects operate to produce corrosion which exhibits itself in the formation of local pitting, terminating in perforation. The writer at one time held this belief and took occasion to make a careful examination of every case of corrosion of this character which came under his notice, with a view of observing whether there was any evidence in support of it. No case, however, has ever been found by him which would support any such view. It has not been found possible to show that tubes which contained such interior surface defects

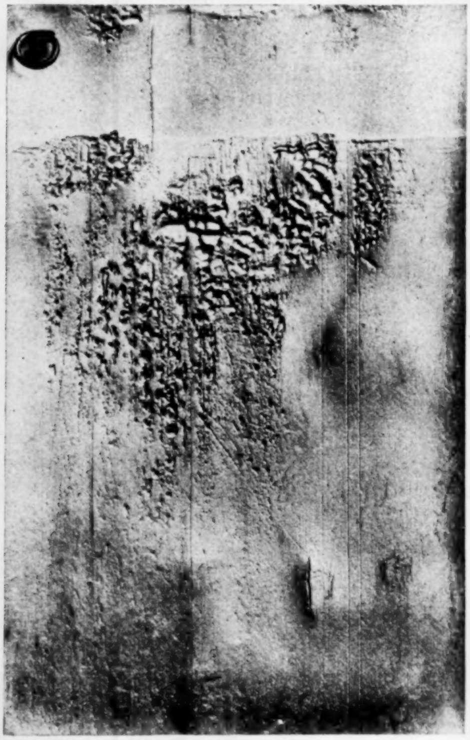

FIG. 2.—Corrosion on Tube Containing Interior Surface Defects.

were any more subject to corrosion than those which were free from them.

It has further been observed that there is no tendency whatever for areas of corrosion to localize in the vicinity of such defects. Moreover, many cases have been found in which severe pitting had occurred in the vicinity of such defects, but absolutely no tendency of the corroded areas to follow along the lines of defect has ever been noticed.

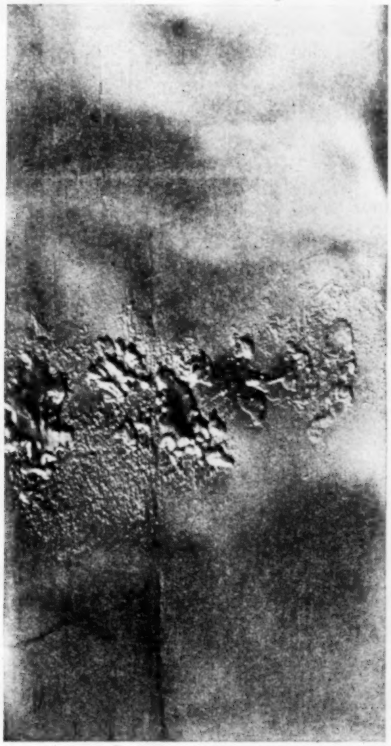

FIG. 3.—Corrosion on Tube Containing Interior Surface Defects.

A recent case of severe corrosion was observed which furnishes very strong evidence that no such connection exists. The tubes had been in service in the condenser of a large stationary plant for a period of six months, and were removed because of perforations caused by local corrosion on their interior surfaces. Of a lot of eleven tubes, eight were found to be absolutely free from surface defects in the vicinity of the corroded areas, while three samples were found to contain such defects.

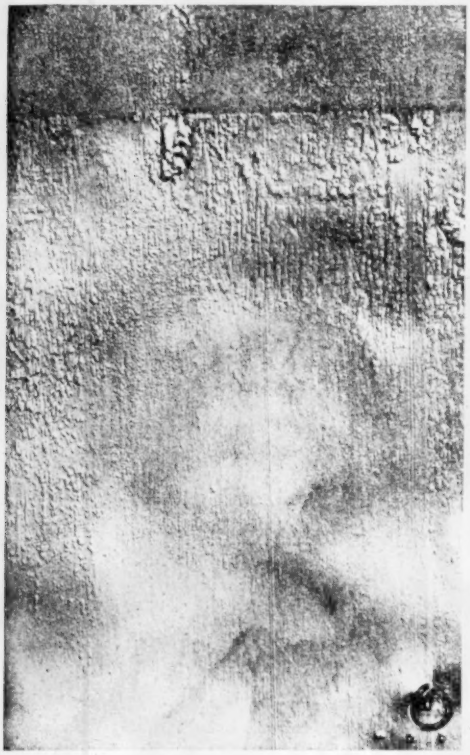

FIG. 4.—Corrosion on Tube Free from Interior Surface Defects.

These tubes were sawed longitudinally and opened out flat so as to show the interior surfaces. Three characteristic samples from the unblemished tubes were taken and photographed for comparison with three containing surface defects, the photographs being reproduced herewith, magnified.[1] These photographs show very clearly the surface defects and the location of the corroded areas with respect to them.

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[1] In preparing these photographs for publication, it was necessary to show them to a slightly reduced size. Figs. 1 to 6 are approximately eight-tenths the size of the tubes.—ED.

Particular attention is called to the fact that even where a corroded area crosses a defect, no tendency whatever for the corrosion to follow the defect is observable. In most of the samples the corroded area was confined to a distance not over 4 in. from the inlet end of the tube; the remainder of the tube being as free therefrom as when first made. In one or two cases the corroded area was similarly confined to a short distance in the length of the tube, but was some distance from the end thereof.

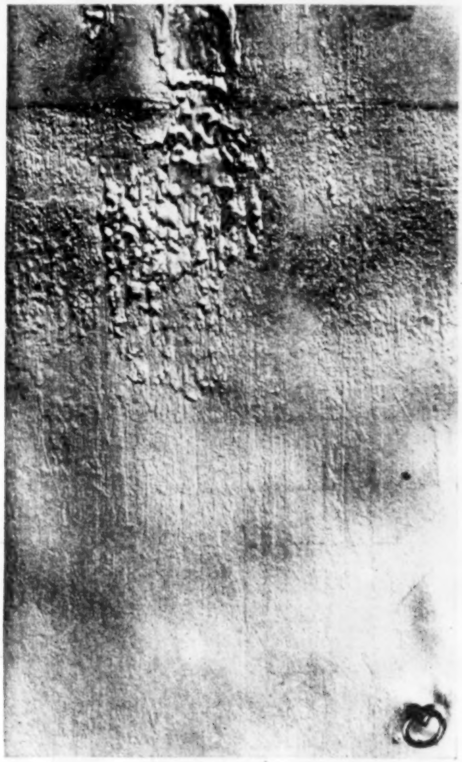

FIG. 5.—Corrosion on Tube Free from Interior Surface Defects.

Figs. 1, 2 and 3 show the tubes containing interior defects, and Figs. 4, 5 and 6 show the tubes which are free from them.

The effect which temperature has upon corrosion is very well shown by the fact that the corroded area in most cases stops quite abruptly on reaching that portion of the tube which is in contact with the tube sheet. There would be naturally a very considerable difference in temperature between the portion of the tube which was in contact with the tube sheet, and that which was in contact with the steam.

FIG. 6.—Corrosion on Tube Free from Interior Surface Defects.

It is not intended that the evidence herewith presented should be considered as arguing in favor of the presence of defects of this character. It is, however, a fact that evidence of their existence can be largely removed by treatment which detracts from the resistance of the tube to corrosion, while on the other hand they are rendered more highly visible by treatment which tends very materially to increase this resistance.

As a consequence, tubes treated in a manner tending to decrease their serviceability will frequently be accepted under specifications containing restrictions of the character in question, but would be rejected when made in accordance with methods calculated to give them the maximum endurance.

It, therefore, follows that a rigidly interpreted clause of this nature may operate to weaken rather than strengthen the specifications of which it is a part.

It is possible that interior defects may be of such a magnitude that they could serve as pockets for the reception of material, either in course of manufacture or in use, capable of inducing corrosive action; in which case it is the foreign matter and not the defect which is the primary cause of the trouble. It is doubtful, however, in view of the large number of observations made, if they exist with sufficient frequency to warrant the condemnation of all tubes containing a defect sufficiently large to be visible.

INSPECTION OF BRASS AND BRONZE.

By Alfred D. Flinn and Ernst Jonson.

SUMMARY.

During recent years many failures of brass and bronze have been noted. These failures have occurred in cast, as well as in wrought metal, which had fulfilled the requirements of standard specifications. These facts indicate that the standard specifications are inadequate.

The service strength of brass and bronze is dependent, not on the ultimate strength, but on the yield point. The true yield point should therefore be specified and a method should be devised for determining the yield point more accurately than can be done by the current method. A test in which the load is applied in increments every five minutes is recommended.

If brass or bronze contains initial stress, the service strength is no longer indicated by the yield point, but by the difference between the initial stress and the yield point. Initial stress must therefore be limited by specifications and suitable methods provided for measuring the initial stress.

It is common practice to "burn-in" defects in brass castings. When a "burn-in" cools it may leave initial stress of such magnitude that the casting will fail locally. Specifications for brass and bronze castings should therefore provide for annealing of all castings which have been repaired by "burning-in."

Molten copper is very susceptible to oxidation. Oxidized brass or bronze is of low strength and gives low elongation. It is recommended therefore that a tensile test with a suitable elongation requirement be incorporated in every specification for cast brass or bronze.

INSPECTION OF BRASS AND BRONZE.

BY ALFRED D. FLINN AND ERNST JONSON.

BRIEF HISTORY.

In 1913, extensive failures of wrought 60–40 brasses, or bronzes, so-called, began to be observed on a number of engineering projects in which these alloys of copper had been used in large quantities, as bolts, rods and plates. These failures have continued to come to the knowledge of the authors to the present time. Recently the failures have not been so numerous, due in large part to the fact that at least in the project where the use had been greatest, the brass had been almost completely removed and steel substituted. Many failures occurred within a few weeks after manufacture, others after various longer intervals, and some not until several years had elapsed. Large forgings made by skillful men, under proper conditions, with correct methods, have proved dependable.

A number of failures of castings, due primarily to improper foundry methods, have occurred, and the form of failure was found to be cracks closely resembling season cracking of wrought brass. These were found to be due in all cases to repairs made by "burning-in" or welding and were proven to be of the same nature as season cracking. A few small castings of a broad "U"-shape failed by cracking, apparently resulting from stresses set up by failure to afford opportunity for contraction during cooling. In the experience of the authors, correctly made bronze castings have been found highly satisfactory, and even some of the most imperfect ones have stood extreme abuse by hydrostatic and other tests without failure other than opening the incipient cracks so as to allow leakage.

This paper has to do with brass and bronze, particularly the 60 copper - 40 zinc class, as used in the more important civil, hydraulic and marine engineering works. The statements made may or may not be applicable to the uses of these alloys and other brasses in the trades. The authors have had only a very limited experience in the latter field and therefore do not venture to apply their observations and suggestions to that field. Naturally these widespread troubles on important engineering works led to thorough investigations, some of which are still in progress. Some of the failures referred to herein, their causes, and the investigations and some of the results and conclusions derived therefrom, have been described in technical publications.[1]

Large numbers of failures of wrought brass have been due to "season cracking", many others to excessive service stresses based on exaggerated claims as to the strength of the metal, which in turn grew out of ignorance or misapprehension as to the real nature of these alloys, and still other failures were due to abuse of the metal in processes of fabrication of the objects from the rods or plates furnished by the original manufacturer. Maker, user and tester alike, apparently were in ignorance as to the real service strength of the alloys under discussion, and even those persons most familiar with them failed to apprehend their true characteristics and the differences between them and the ferrous metals, particularly the common steels. Following the steel precedent, the same methods of testing and similar forms of specifications have been used hitherto. Until about 1912 or 1913, the relatively small quantity of these alloys used in engineering structures, and the scattered failures, did not cause anyone to question the methods and results commonly accepted. Even those who were familiar with "season-cracking" and had given some attention to it, failed to see its true significance, although it is this type of failure which has ultimately led to the clearer view of the problem and the discovery that the copper alloys have different relations between service stresses and results of quick physical tests such as have been commonly used for steels, and require different methods of testing.

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[1] *Technical Paper* No. 82 of the Bureau of Standards, by Paul D. Merica and R. W. Woodward, entitled "Failure of Brass;" "Brass in Engineering Construction," by Alfred D. Flinn, ***Engineering Record***, Vol. 68, 1913; "The Cracking of Brasses and Bronzes," by Van Aken, ***Engineering Record***, Vol. 70, 1914; "Troubles with Forgeable Brass," by Ernst Jonson, *Transactions*, Am. Inst. of Metals, 1914; "Brass in Engineering Construction," by Alfred D. Flinn, *Proceedings*, Municipal Engrs., Nov., 1914; "Fatigue of Copper Alloys," by Ernst Jonson, *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 101 (1915); "Some Experiences with Brass in Civil Engineering Works," by Alfred D. Flinn, *Proceedings*, Am. Inst. of Metals, Oct., 1915; "Notes on the Inspection of Bronze and Brass," by Ernst Jonson, *Proceedings*, Am. Inst. of Metals, Sept., 1916.

GENERAL CONSIDERATIONS.

It has become evident that these brasses would not endure the hard usage to which many steels are commonly and constantly subjected with impunity. Nevertheless, some manufacturers had been claiming greater strength and toughness for their "bronzes" than steel possessed, in addition to incomparably greater resistance to ordinary corrosion. As thus represented, these alloys were almost ideal for many purposes in hydraulic and marine work, and at the low prices prevailing before 1914, they could be used somewhat freely in important work without extravagance in ultimate cost.

Early in the experience of these troubles with brass and the investigations which followed, the authors became convinced that methods of inspection and test, which have continued in use to the present time, were totally inadequate and misleading. Much wrought brass which failed very soon after manufacture passed the standard tests most acceptably, and after failure, specimens cut as close as practicable to the place of failure, would pass all the standard tests equally well. To supplement improved processes of manufacture and fabrication and to discourage unsatisfactory processes, practical methods and means of inspection and test must be devised which will reveal the true strength and dependability of the metal at any time after its manufacture or its fabrication into articles for use. No such tests have yet come to our knowledge which are commercially feasible and otherwise wholly satisfactory, although many have been suggested. The purpose of this paper, therefore, is to present this important problem so that a solution may be found.

Closely allied to the problem of inspecting and testing, are correct forms of specifications for the several alloys, according to their uses. The investigations have not yet proceeded far enough to make it possible to write new specifications which can be regarded as final. The best current specifications for brasses and bronzes are those of the U. S. Navy Department. These specifications, however, cover only some of the important properties of these materials. There are other highly important properties which are not regulated by these specifications. These are the true yield point, initial stress in drawn brass, cooling stresses in "burnt-in" brass castings, and oxide in bronze castings.

TRUE YIELD POINT OF BRASS AND BRONZE.

Brass and bronze, and probably other copper alloys, fail by cracking if exposed for any considerable length of time to a tensile stress which is greater than the yield point of the material.[1] The safe working stress which is to be used in construction must, therefore, be determined with reference to the true yield point of the material. The ultimate strength should be known, but it has little to do with the determination of the working stress. This fact calls for two essential features which must be possessed by an adequate specification for these materials. The first of these essentials is that the true yield point must be specified and its exact determination provided for. The second is that initial tensile stress of appreciable magnitude should not be permitted, and the detection of such stress should be provided for. Neither of these requirements is sufficient by itself. A high yield point signifies nothing unless it is known what portion of this yield point is cancelled by initial stress and therefore not available for working stress.

When the yield point of a copper alloy is reached, the metal flows until it is sufficiently hardened to carry the load. This flow takes an appreciable length of time. It is impossible, therefore, to determine the true yield point by means of a test in which the specimen is continuously stretched. In order to find this point, the load must be applied in increments and the beam of the testing machine must be kept balanced so that it will show by its downward movement when the first flow takes place. The smaller the increments are made, the more accurate will the determinations be. Each increment should be given at least five minutes in order that there may be no doubt about the capacity of the material to hold the load without yielding. There should be also a pointer and a scale at the end of the beam to make the movement of the beam more easily observeable. Occasionally a sensitive extensometer applied to the specimen will check the indication made by the end of the beam. For acceptance tests, however, the observation of the drop of the beam is probably the more practical method.

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[1] Ernst Jonson, "Fatigue of Copper Alloys," *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 101 (1915).

WROUGHT BRASS.

Initial Stress.—Whenever a rod, bar, tube or shape of brass is drawn through a die, the permanent reduction of its dimensions is proportionately greater at and near the surface than in the interior. This is proven by the fact that the diameter of a drawn bar may be increased by squeezing it so as to cause the metal in the surface layer to stretch, thus allowing the elastically compressed interior to expand. The stress set up by the processes of manufacture on the surface of drawn brass frequently exceeds the initial yield point, and cracking may be expected after some time when corrosion takes place. This is the reason why this defect is called "season cracking." Drawn material should, therefore, not be allowed to retain its initial stress for any considerable length of time, but should be immediately treated in some way to relieve the stress.

Initial stress may be relieved in two ways; either by stretching the metal near the surface by mechanical work, or by temporarily reducing its elastic limit by heating, and thus allowing it to be stretched by the compressive stress in the interior metal. The mechanical working used for this purpose is either squeezing between the rolls of a straightening machine or bending the bar successively in four directions, called "springing." Annealing is used to some extent to eliminate intitial stress by lowering the elastic limit. An important fact with regard to annealing drawn brass was brought out by Merica and Woodward in the paper which they read before the American Institute of Metals at its meeting in 1915. The fact is that if sufficient time is allowed for annealing, so that the metal is given ample opportunity to flow, the temperature need not be as high as it would have to be in order to eliminate initial stress by quick annealing. It is therefore possible to eliminate initial stress by slow annealing at a low temperature without appreciably lowering the yield point given the material by the drawing. The work done by Merica and Woodward in connection with the study of initial stress and "season cracking" has been published in full detail in Paper No. 82 of the Bureau of Standards.

Method of Measurement of Initial Stress.—The following method for determining the initial stress is suggested:

Test drawn rods, bars and tubes for initial stress by splitting on a diametral plane for a length not less than four diameters. When split to this depth, the dimension at the end of the rod perpendicular to the cut should not be increased more than $x/1000$ part. (The value of x is to be determined by tests of the kind proposed above, running a parallel series with tests proposed by the Bureau of Standards.) When one main dimension of the cross-section is greater than the other, the cut should be made in the plane of the greater dimension. When material is taken from stock, one test should be made on each end of each piece, except in the case of extruded material which has not been trimmed, which need be tested only on the rear end. If the cold working be done in the presence of an inspector, it will suffice if 5 per cent of the material be tested. If the material be extruded in the presence of the inspector, the test on the front end may be omitted. The rods to be tested should be selected by the inspector. In making the selection, the inspector should include the rod which is the last one in the final operation. If one specimen fails when a lot of material is tested in this way, the whole lot should be rejected. Material thus rejected may be retreated and retested.

This method may be checked by the following one worked out by the Bureau of Standards:

(The figures below are given on the basis of a 10-in. strain gage; in parentheses are given the corresponding figures for the use of an 8-in. gage.)

(*a*) *Round Rods.*—Gage points are laid off along two opposite fibers, beginning $1\frac{1}{2}$ in. from the end of a $12\frac{1}{2}$ ($10\frac{1}{2}$) in. test sample of the material. Within the gage points and over a length of 9 (7) in., the rod, carefully centered, is machined in the lathe to 0.706 of its original diameter. Care is taken (1) that the specimen is not thereby heated above about 100° C. (212° F.), and (2) that the specimen is not gripped by a dog within $\frac{3}{4}$ in. of a gage point. The average change in length registered by the strain gage is measured and the average initial stress calculated by the formula

$$\text{Stress} = \frac{\text{Modulus of elasticity}}{9\ (7)} \times \text{change in length}$$

(*b*) *Rectangular Bars.*—The same procedure is followed except that the specimen is milled symmetrically on the two pairs of opposite sides successively, to one-half of its original cross-sectional area. The average stress is determined from the same relation as that used for round rods.

Rolled rods should always be annealed after drawing because, owing to the lack of uniformity in their section, an initial stress test made on one end or even one on each end would not guarantee no initial stress existing at intermediate points.

Extruded brass rods are sometimes subject to a hidden defect, namely, piping. Such rods are generally cut to length by sawing, and if the pipe is small, it will be hidden by the rubbing of the saw. The presence of this defect may be discovered by knicking the end of the rod and breaking it off.

In the case of wrought material, the choice of process to meet specifications might safely be left to the manufacturer, because the presence of initial stress may be detected by tests after the material is finished, and the specifications should provide for such tests.

BRASS AND BRONZE CASTINGS.

Foundry Repairs.—Brass and bronze castings are expensive to make and it is therefore undesirable to reject such castings on account of defects which can be remedied. Minor leaks in hydraulic castings may be stopped by peening, but the fact that the casting leaks at a certain point generally indicates that the metal is defective at this point. If water comes through the walls of a casting even in very minute quantities under pressures which do not exceed 1,000 lb., this is an indication that the metal is not clean, or that the casting is porous from some other accidental condition. Peening is therefore a questionable method of treating defective spots in brass or bronze castings. If the defect is small and other circumstances permit, a hole may be drilled and a plug of the same metal as the casting may be screwed in. If plugging is not practical or permissible, defective spots should be cut out by chipping or drilling so that all the defective metal is removed. The cavity may then be filled by pouring metal into it from a crucible or by melting rods of metal of suitable composition in place, using a gas flame. In either case this must of course be done in such a way that the entire surface of the cavity is melted and thus consolidated with the added metal. If a gas flame is used, there is some danger of the filling not being solid, but if the filling is poured from a crucible with reasonable care, there can be little doubt about the soundness of the metal in the weld.

Cooling Stresses.—As previously stated, brass or bronze when stressed above the initial yield point for a considerable length of time, will fail by cracking, when oxidation takes place on the surface, which will happen sooner or later. When a defective spot in a casting is welded, the cooling of the metal in the weld will be accompanied by contraction which will put a tensile stress in the metal of the weld as well as in the old metal which surrounds it. If the metal of the weld is the same as that of the casting, its elastic limit may be appreciably higher than that of the rest of the casting, because the metal in the weld solidifies much more rapidly than the rest of the casting did originally. The permanent deformation resulting from the shrinkage of the weld will, therefore, take place in the surrounding metal, and this metal will remain in a state of tension greater than its initial yield point and after a time cracks will appear. The magnitude of the stresses thus caused may be estimated from the fact that the shrinkage of brass from the freezing point down to ordinary temperature is about $\frac{3}{16}$ in. per ft. or about 1.5 per cent. If the elastic limit were high enough, this shrinkage would produce a stress of not less than $14,000,000 \times 0.015 = 210,000$ lb. per sq. in. In other words the metal surrounding the "burn" will be stressed beyond the yield point. The "burn" itself, while stressed as high as the surrounding metal, may not be stressed to the yield point because, being chilled from pouring into the cold casting, it gets a higher yield point than the same metal cast in sand.

If the casting is of small lateral dimension, and the ends are not constrained, and the break extends all the way across it, the stress set up in the old metal by the shrinkage of the weld is compressive, and therefore there is no danger of cracking. In other cases shrinkage stress must be prevented by keeping the casting heated to a very high temperature while the weld is being made and until it has solidified. The yield point of the surrounding metal will thus be temporarily lowered and the flow will take place under a stress much lower than the yield point of the cold metal, so that there will be no danger of cracking after the metal has cooled. Another way to prevent cracking is to anneal the casting immediately after the weld has been made. There is no reason to believe that the metal surrounding the weld is injured by the shrinkage stress until corrosion occurs on its surface. Therefore, if the yield point of the metal surrounding the weld is lowered by heating the entire casting to a sufficiently high temperature, additional flow will occur, and the cooling stress will thus be gradually reduced to a very small minimum, according to the length of time the annealing is continued. The annealing temperature should be maintained for several hours so as to give the metal time to flow.

SPECIFICATIONS AND TESTS FOR CASTINGS.

There should be definite specification requirements to control the "burning-in" process. The specifications for manganese bronze castings should cover this point in such a way that absolutely safe castings will result, even when defective places have been "burned-in," merely by putting the specifications into the hands of a careful and experienced metal inspector who has no special familiarity with brass.

It now appears that the only way to insure safety in brass (bronze) castings is to specify annealing at a certain temperature for a certain length of time whenever "burning-in" has been done.

OXIDE IN BRONZE CASTINGS.

Brass and bronze castings are subject to various defects which are difficult to discover by surface inspection, or even by hydrostatic testing. The defect which most commonly occurs results from the inclusion of oxide in the metal of the casting.

This occurs in two ways; either the molten metal contains an admixture of oxides, owing to insufficient protection of the molten metal from the air, or the dross from the surface of the crucible or furnace charge gets into the mold and is caught at some point where the flow is slight, and is thus prevented from coming to the surface in the risers. In the former case the entire casting is bad, and the best way to discover this defect is to make tensile tests on specimens cut from a coupon cast from the same melt. The admixture of oxide is indicated most distinctly by the greatly reduced elongation, but also by low ultimate strength. The tensile test should, therefore, be specified for all important brass and bronze castings, no matter whether a certain strength and elongation are of mechanical value or not. The metal in every important brass or bronze casting should have the strength and elongation typical of the mixture of which it is made as an evidence that the metal in the casting is clean. If a tensile test is impracticable, oxidation in the metal may be discovered by making a bending test on a machined specimen. The presence of oxide will then be indicated by a number of small cracks which open on the outside of the bend, and if the oxidation is extreme, also by the abnormal color of the surface of the fracture.

Experience of the authors indicates that oxidation of metal in the crucible is a very common defect, especially of the mixtures containing high percentages of copper, that is, of the bronzes. It is the belief of the authors that in foundries where tensile or hydrostatic tests are not made, the metal is generally allowed to become considerably, and in many cases seriously, oxidized in the crucible, because this defect in the metal does not show on the surface of the casting. To specify bronze castings merely by the mixture is therefore useless. Unless the metal is tested after it has been poured, one is not justified in assuming that it consists of a clean uniform mixture of the ingredients put into the crucible. A bronze casting may be made of the correct mixture and may show no surface indications of defects, and still it may be nothing but a honeycomb of metal, filled with oxide.

The presence of included dross is more difficult to discover. If a hydrostatic test cannot be made, it is practically impossible to find such defects unless they happen to come to the surface of the casting, and even a hydrostatic test does not always discover such defects, because there may be a considerable thickness of good metal along one or both of the surfaces of the casting. The best insurance against this kind of defect is correct molding, a thing which, strange to say, is unusual.

FURTHER INVESTIGATIONS.

The Bureau of Standards, Department of Commerce, Washington, D. C., has become the clearing house for accumulating information on this very important subject. Readers of this paper, therefore, are urged to send to the Bureau any information or suggestions which may contribute to better methods of manufacture, or suitable specifications and effective means of inspection and test. In the above the authors have ventured some suggestions, not so much with the expectation that they state methods which will prove to be final, but rather that these methods may be tried by other investigators, and especially by manufacturers. It is hoped that in this way in the near future the desired results may be achieved.

DISCUSSION.

Mr. Flinn. MR. ALFRED D. FLINN.—Our extended, unfavorable experience in the use of wrought bronze and brass, particularly manganese bronze and naval brass, for bolts, ladders and various other forms, in large hydraulic work, has led to the almost complete removal of those parts and the substitution of steel with or without galvanizing or other protective coating. It is possible that if there had been known practical and dependable tests by which metal which would continue satisfactory could be distinguished from that which would fail, substitution would not have been so general; but with failures among the products of every manufacturer, there was nothing left for the engineers, in view of the lives and property dependent upon these pieces of metal, except their elimination. A number of tests have been suggested. Some of these are not practical for everyday use, such as modifications of Dr. Heine's method of determining initial stresses by turning down or boring out specimens of wrought brass. A method for rods not described in this paper, is to cut from a specimen about 6 in. long a longitudinal, sector-shaped slice and observe its curvature.

If wrought brass and wrought bronze are to be used under stress in engineering works of importance, some treatment must be applied to them which will remove uncertainty as to their dependability in service, and with that must go some means of testing, simple and dependable, which can be put into the hands of a reasonably intelligent inspector such as would be employed in ordinarily well managed engineering works.

This further difficulty arises in practical testing, that brass is usually bought in such relatively small quantities that one cannot afford to have an inspector constantly at the numerous brass works which may be producing metal for him. In other words, the inspector first encounters the metal when it is piled up in the storeroom ready for shipment. He must take from the large number of pieces specimens which will be representative if possible. Our observation has shown that it is often impossible to select a specimen or a number of specimens which will be representative, because even in one given bar the degree of trouble may vary from end to end. It is expected that manufacturers and investigators familiar with these alloys will soon be able to get some definite, workable means of inspection and accompanying specifications. **Mr. Flinn.**

We found also that practice, even in experienced foundries, was often faulty. This led to endeavors to save castings of considerable value by "burning in" or welding by other methods. This repairing caused failures similar to those in wrought metal. This has led to the suggestion that castings that have been subjected to such repairs should be annealed, and possibly that such repairs should be made only to castings which are pre-heated and kept at a considerable degree of heat while the "burning-in" or welding is being done, that the heat should be maintained for a number of hours subsequently, and that the cooling should be slow. Some practical tests on large castings have shown a large degree of success with the oxy-acetylene flame. Knowing that such repairs are sometimes made surreptitiously, perhaps even unknown to the management, by workmen who wish to cover their own faults, all important bronze castings should be annealed before acceptance.

It is desired that members make definite suggestions which will aid in writing improved specifications, which, even if tentative, will lead to better practice.

MR. E. L. LASIER.—I have been much interested in the authors' paper, especially in their discussion of initial stresses. **Mr. Lasier.**

Doubtless many present will be interested to know that the Navy Department's specifications for rolled naval brass (or composition n-r) bars, shapes, sheets, plates and rods—presumably the specifications referred to on the fourth and fifth pages of the authors' paper—have recently been revised and will soon be issued by the Department with the inclusion therein of provisions dealing with the subject of initial stresses. These specifications cover an approximately 60-40 composition, minimum tensile strength 54,000 to 60,000 lb. per sq. in., yield point 25,000 to 28,000, and elongation in 2 in. of 30 to 40 per cent. The material is used in the Navy as a general structural brass.

Three insertions will appear in the new Navy specifications.

Mr. Lasier. First, there will be the requirement that a 6-in. test length of the material shall be immersed for ten minutes in a mercurous-nitrate solution proportioned 100 g. of mercurous nitrate and 13 cc. of 1.42 sp. gr. nitric acid per liter. Upon removal of the test samples from this solution, no cracks shall be visible. Second, there will be a suggestion to manufacturers that their material be sprung by passing it through a suitable straightening machine in the endeavor to remove any initial stresses which may be present in the material; this procedure, however, will be suggested only, and not, at least at this time, be made obligatory. Third, there will be instructions to Navy inspectors to take a sufficient amount of the material offered for inspection, to permit the measurement of initial stresses substantially by the method which the authors describe as having been "worked out by the Bureau of Standards;" the measurements to be made, however, will have no bearing whatsoever, at this time, on the question of the acceptance or the rejection of the material by the inspector, but will be made solely for purposes of information and record, to ascertain whether the prevailing stress in the outside layers is tension or compression, and the value of that stress in pounds per square inch. Relative to this third insertion, probably the only substantial difference from the requirements contained in the authors' paper will be the addition in the proposed Navy specifications of a third "precaution" to be taken, namely, that "at all times a light cut, not over 0.005 in., shall be adhered to."

The Navy's investigation which has resulted in this specification, now about ready to be issued, has of course been approached from several different viewpoints. For instance, the question of a required annealing was considered. This undoubtedly would be the most satisfactory method of eliminating initial stresses present in structural brass of the composition and physical requirements called for by the Navy specifications. Since, however, a very low temperature would be required to permit the material to comply with the physical requirements, and since manufacturers in general are not equipped to anneal at the low temperature necessary, this proposition was not given much further consideration because of the resulting increase in the cost of material.

Mr. Lasier. The requirement that the material shall satisfactorily withstand immersion in a mercurous-nitrate solution will probably prevent the acceptance of material containing dangerous initial stresses, but it is doubtful whether it will prevent the acceptance of material containing initial stresses which may be dangerous *in combination with* the stresses to which the material will be subjected in service. For instance, I was interested in a recent paper on brass and other copper alloys used in marine engineering, presented last month by Mr. T. P. Milton before the Institute of Marine Engineers. Mr. Milton described a shipment overseas of some bars, accepted by Lloyds' inspectors, specified to a tensile strength of 30 tons and an elastic limit of 20 tons per sq. in., which, upon delivery, showed 75 per cent of the shipment containing cracks which had developed in transit. Such cracking was undoubtedly due to initial stresses. Presumably immersion in a solution of mercurous nitrate would have guarded against the acceptance of such material, since the cracking occurred previous to the subjection of the material to any service stresses.

The required measurement of initial stresses, as appearing in the specifications to be issued by the Navy Department, is purposely stated "to be made for information and record only" at this time because of several questions still involved. It is doubtful if we know the value of the maximum initial stress which may be present with safety in structural brass. The Bureau of Standards has suggested 10,000 lb. per sq. in. based on their work; that may or may not be a good working value. The results obtained at this time in measuring initial stresses are far from concordant. There is, too, the question as to whether the turning down of a sample springs a rod so as to change considerably the initial stress. Still another question is, exactly what is the difference between an unsprung rod and a sprung one? And a fifth and important question involved is the lack of knowledge as to the real causes of longitudinal cracking versus transverse cracking. Manufacturers have suggested that the greatest proportion of failures is due to longitudinal cracking. Just what is the relation between the initial stress producing longitudinal cracks and those stresses which will produce transverse cracks? In the paper by Mr. Milton, to which I just referred, attention is called to the examination of some etched sections of brass

Mr. Lasier. bolts used as connections on a large ship. Those bolts cracked spontaneously when placed on the ship. The etched sections showed that the longitudinal cracks were separations of the crystals and the cross cracks were fractions of the crystals. Just what the relation may be, of course, is problematical.

Mr. Bassett. MR. W. H. BASSETT.—I should like to ask Mr. Lasier if, in the experience of the Navy Department, there has been any considerable trouble of a nature similar to that experienced by Mr. Flinn, either under the present specifications 46-B-6-B, or under the former specifications 46-B-6. The tensile strength and elastic limit were somewhat higher in the older specifications, and it would be very interesting to know what the experience has been, if it can be ascertained, both in the interest of engineers and in the interest of manufacturers of material.

In regard to the question of the springing of material, which was brought up as a suggestion, we believe it would be better if it had rather more force than a mere suggestion. It would be better if it was made a requirement.

The interest in Mr. Flinn's trouble has induced, among the manufacturers, considerable testing of material to find out, if possible, what was the cause of cracking such as was experienced. A great many specimens have been tested in mercurous-nitrate solution, and corresponding ones exposed out of doors and close to the surface of water for a whole year around, so as to have the influence of winter temperature, and at the same time of an atmosphere nearly saturated with moisture. It is apparent that moisture has considerable to do with the trouble. In no case have samples corresponding to those which resisted the mercurous-nitrate test cracked in such exposure.

We have no very definite knowledge in regard to the relation between cross cracking and lengthwise cracking, but one point which became very evident from samples which Mr. Flinn kindly let me have for testing was, that the manganese bronze in many instances cross cracked, while naval brass showed a tendency to crack lengthwise. This raises the question whether rods carrying low copper—in other words, those which are rich in the so-called Beta constituent—are not the ones which give all the trouble due to cross cracking.

Mr. Webster.

MR. W. REUBEN WEBSTER.—Experienced brass makers are usually reluctant to furnish material to specifications analogous to those applying to structural materials such as steel. This feeling is prompted by the belief that insufficient data of an accurate and reliable nature exist in technical literature to permit one, however highly informed on other subjects, to cover the matter in a satisfactory manner. Manufacturers prefer to trust the results of their own experience as expressed in successful manufacturing methods, rather than accept the specifications of users unfamiliar with the various properties of the material. This paper is highly welcome, therefore, as illustrating the necessity for comprehensive knowledge before determining by inspection and test the suitability of particular material for a specific use.

In this connection it is the speaker's belief that brass or bronze could have been furnished which would have exhibited few or no failures in the situations referred to in this paper, but which would have failed to pass the specifications imposed. Large quantities of brass and bronze are employed under severe service conditions with no more failures than are experienced with any other material.

In the interests of good engineering practice, however, it is desirable that acceptable methods of inspection and test be adopted. Some of the proposals in this paper are, however, open to question. In particular, careful experiments should be conducted to determine the sufficiency of the method suggested for determining the yield point.

It is further felt that the whole matter of initial stress is still far from understood. The investigations of the Bureau of Standards have added much to the knowledge of the subject, but sufficient is not yet known to permit prediction of the extent and distribution of initial stress from a knowledge of the methods of manufacture. Very little has yet been done to determine the effect of springing on initial stress or the extent to which it reduces the danger of season cracking in case of rods properly so treated.

In this connection it is desired to show the effect of this operation in two specific cases.

In connection with a recent investigation a high brass rod

Mr. Webster. was extruded to 2 in. in diameter, drawn to $1\frac{15}{16}$ in. and carefully annealed. It was then cut into two pieces, one of which was drawn to approximately $1\frac{13}{32}$ in. in one pass, the other to the same size in five passes. A portion of each rod was then thoroughly sprung in a straightening machine of the Medart type. Samples of all four rods were then submitted to the Bureau of Standards for determination of initial stress. Fig. 1 shows one rod (Specimen No. 1) as drawn and the same rod after springing. Fig. 2 shows the other rod (Specimen No. 5)

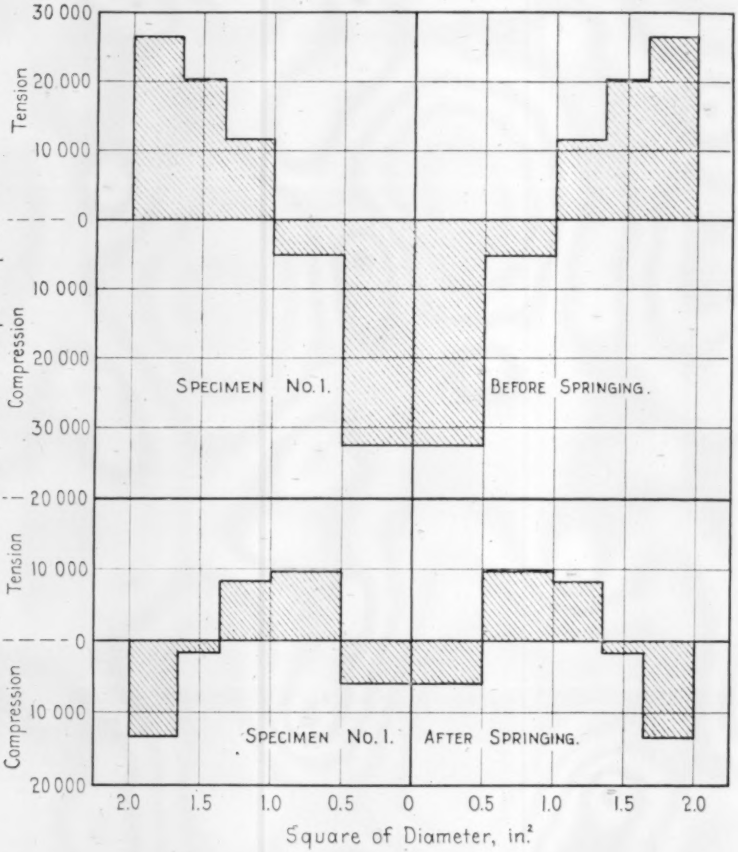

FIG. 1.

Mr. Webster.

after drawing and after springing. A doubt exists as to which specimen was drawn in one pass and which in five, but because of the fact that the springing operation increased the rod diameters several thousandths of an inch, no doubt exists as to which rods were sprung and which were unsprung.

I desire to call particular attention to the fact that in all the discussion of brass failures excited by the New York aqueduct experiences, the extreme importance of springing as a preventive measure has been unrecognized, although brass makers of experience have been entirely familiar with the fact. No better proof of the inadequacy of the literature on brass as a basis of specification writing could be instanced.

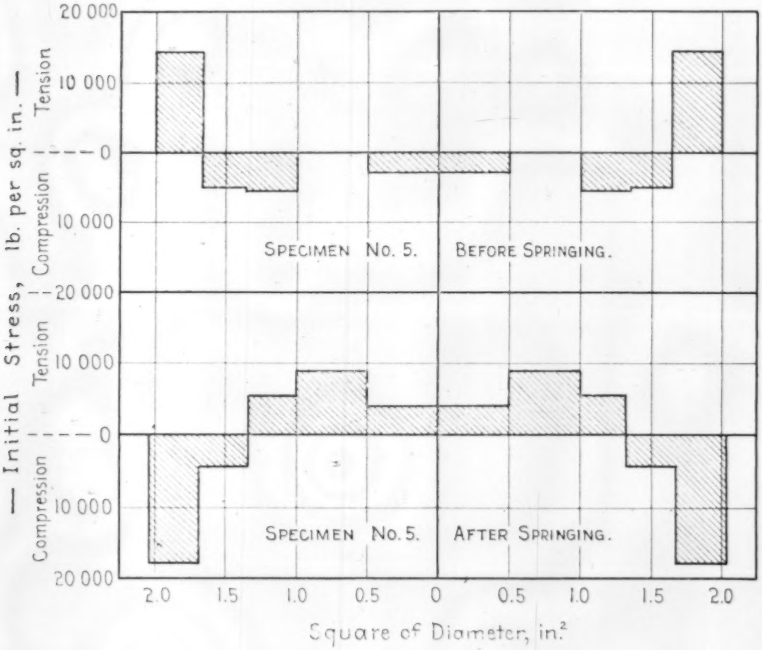

FIG. 2.

I also desire to call in question the statement that "rolled rods should always be annealed after drawing because, owing to the lack of uniformity in their section, an initial stress test made on one end or even one on each end would not guarantee

Mr. Webster. no initial stress existing at intermediate points." A rolled brass rod is usually made from a round or rectangular casting, having a uniform cross-sectional area throughout its entire length, so that the degree of reduction effected by the rolling is likewise uniform throughout the length of the finished rod.

I desire to agree with Mr. Bassett in the feeling that the requirement of a thorough springing, as a final operation for material of this character, is exceedingly desirable. Some samples will be prepared and submitted to the Bureau of Standards in the endeavor to get additional information concerning the effect of various methods of producing rods and processing them on the distribution of initial stresses.

Mr. Lasier. Mr. Lasier.—In reply to Mr. Bassett's question as to the experience of the Navy Department, so far as I am aware, we have had no actual trouble; it is merely a question of profiting by the troubles of others and hence trying to prevent them.

I think both Mr. Bassett and Mr. Webster will bear me out in saying that it is undoubtedly necessary to subject rods to some cold work in order to have them meet the tensile requirements of the Navy specifications. Probably sheets and plates which come under the specifications in question can be rolled to meet the requirements without any cold treatment, but I do not believe rods can be. It is also probably necessary to draw rods through dies to maintain their diameters; otherwise they would be very non-uniform in diameter.

In so far, however, as the experience of the Department is concerned, we have had no serious trouble with season cracking.

Mr. Lindsay. Mr. C. F. Lindsay.—The following experience, while not pertaining to the class of material being discussed, may be of interest. In 1911, one of the large cartridge-case manufacturers in this country made up several thousand 4-in. Mark VII Argentine cases with a very severe reduction at the mouth. Those cases looked so pretty that the manufacturers saved a small percentage of them, which were stored aside in a nice, clean atmosphere, in dry rooms. At the end of over two years, when more than 90 per cent of the shipment was returned on account of season cracking, not one of the samples kept in that dry atmosphere had cracked. These cases were then examined and found perfect; but in 24 hours, by exposing them to corrosion, every one of the cases cracked.

Mr. Olfs.

MR. A. C. OLFS.—I should like to ask, before the discussion ends, whether some one present can state whether the composition of the metal plays any part in the question of cracking. In the study of this question we have analyzed a good many samples of manganese bronze, and so far as we could see, there was no particular difference in the composition of specimens that stood exposure without cracking, and those that did not. We did not examine for such elements as bismuth, cadmium, arsenic or a number of other metals that might have been present in very minute quantities and have played some part, conceivably, in the cracking; but if any one present can throw any light on this question, I think it would be a step in advance in our knowledge on the subject.

Mr. Bassett.

MR. BASSETT.—Perhaps Mr. Flinn could answer that question better than I, but as stated before, Mr. Flinn kindly furnished samples for investigation. Antimony, bismuth and cadmium were very carefully looked for. In none of the samples was any antimony or bismuth found, although very careful analysis of large samples was made. A very small amount of cadmium was found in some samples, while in others no cadmium at all was found, so that no definite conclusion could be drawn as to whether or not cadmium had played any part in the matter. The matter of the determination of cadmium has been questioned, but we are very sure that some of the samples Mr. Flinn furnished were absolutely free from cadmium, because we spent a great deal of time and effort in order to make sure of finding and determining any very small amounts of cadmium which might have been present.

There is another consideration that requires attention after hearing Mr. Lasier's reply in regard to the Navy Department's experience, that they have had no trouble in their use of brass. If the brass material is annealed, of course the elastic limit becomes very low; lower than is often supposed, because its determination depends upon the accuracy of the instruments used, and the elastic limit employed by engineers in their calculations is frequently higher than the true value. If material of low elastic limit is specified—lower, perhaps, than the engineer supposes on account of misleading determinations—and put into service, it may be strained beyond its elastic limit. This results

Mr. Bassett. in a new and higher elastic limit and tensile strength being set up. Although there may be no internal strains in the material when it is supplied if annealed material has been required, is there not danger when such material is put into service, that it will be overstrained, and that internal strains will be set up, and may these not bring about such trouble as we are trying to guard against?

Mr. Elliott. MR. G. K. ELLIOTT.—I hope I may be pardoned if I divert the focus of your attention for a few minutes from the subject of wrought metals to that of castings, which we find is also touched upon at considerable length in the paper before us. I wish in particular to refer to that part of the paper which deals with the subject of oxides in bronze castings. In speaking of the hydrostatic test in this connection the tendency of the authors' paper is to lead one to the belief that a casting leaking under the hydrostatic test is rather sure to be contaminated with oxides, or in other words, that the hydrostatic test is a reliable means of identifying oxidized bronze. That this is not a reliable test is my firm belief founded upon many examples which have accumulated from the almost daily experience of seeing some 20,000 to 30,000 lb. of bronze castings of various alloys tested hydrostatically, although I recognize the frequency with which oxidized metal occurs in castings and its importance as a disturbing constituent in such castings.

One experience I shall recall. It had to do with bronze castings made of an alloy containing about 7 per cent tin, 4 per cent zinc, and a small amount of lead. These castings were of one of those designs which are the despair of the foundryman and were almost impossible to cast with good results, the outcome being that practically all of them leaked under test. The metal had received exceptional and careful attention from beginning to end and, important to record here, had been melted in an oil-burning furnace with a reducing flame so that there was a minimum chance for oxidation. Nevertheless the castings leaked freely. The final of many remedies tried was that of annealing for a short time at about 1400° F. Upon retesting the annealed castings it was found that more than 75 per cent of those which previously had leaked were now tight under the hydrostatic test. Microscopic examination of several of the annealed castings revealed the fact that they had a very homogeneous structure consisting of what probably was the Alpha constituent, whereas the original untreated castings were found to be decidedly heterogeneous with very prominent segregations of the eutectic which probably was the copper-tin compound Cu_4Sn. The effect of the annealing was to dissolve the eutectic, leaving a uniformly unbroken homogeneous structure. It was the elimination of the eutectic structure that overcame the porosity of the castings. Oxidized bronze that leaks under test I do not believe can be corrected by any method of heat treatment. In considering this subject, therefore, we should bear in mind the fact that oxidation is not the only considerable cause of porosity to be reckoned with, but also that almost equal in importance is heterogeneity of structure, especially that which is manifested in well-defined eutectics. **Mr. Elliott.**

Speaking further of the effects of the annealing experiment mentioned, it may be of interest to mention that the annealing had the additional beneficial effect of perceptibly increasing the elongation of the metal while the ultimate strength remained unchanged.

MR. N. K. B. PATCH.—While we are on the subject of sand castings, I should like to ask Mr. Flinn if he, as an engineer, has considered the troubles of a foundryman by reason of faulty design on the part of engineers. The foundryman is striving to mold castings correctly and accurately, but the engineer sometimes seems to design in a way to trouble the foundryman as much as possible. The location of thick and thin walls in immediate proximity often cause the foundryman a great deal of thought and worry that could be entirely avoided if the engineers designed the castings correctly. The inability to feed by risers or to bring gates to heavy spots is often the cause of trouble later, due to internal strain set up by irregular cooling. It would, therefore, seem that if the engineer would cooperate with the foundryman, getting the foundryman's advice as to the best way to design the casting, many of the troubles referred to by Mr. Flinn would be prevented before the castings were ever made. **Mr. Patch.**

It might also be pointed out that such cooperation would often save expense and pattern work in molding, as well as the expense and trouble due to failures.

Mr. Flinn. MR. FLINN.—To answer the question just raised by Mr. Patch, it will interest you to know that important castings were not put out for bids by the Board of Water Supply engineers until not one, but several of the best-known brass foundrymen in America had been consulted. Not only that, but after these castings had been made, the designs were submitted to other good brass foundries and the Navy Department, and the only suggestion of change any one made was in the bonnet castings for very large gate valves, 30 and 48 in. in port diameter; instead of putting both sets of ribs (horizontal and vertical) on the inside of the bonnet, as had been done, one man suggested that one set be inside and the other outside.

In answer to a suggestion by an earlier speaker, not only were these specifications and designs for castings considered with manufacturers, but in preparing all specifications, including the wrought material, manufacturers were asked to make suggestions. Six or seven years ago, of two opposing sets of manufacturers, one stoutly insisted that the tensile strength should be increased materially above that specified and the other that it should be reduced.

As to the effect of composition, the most interesting general comment obtained from extensive inquiries was that of the Philadelphia Navy Yard, which for some years has been making a very large proportion of the manganese-bronze castings for the Navy. A successful and experienced foundryman there insisted upon the purest of ingredients for manganese bronze—the best of copper, and only the highest grade of zinc, such as "Bertha" or "Horsehead." Without difficulty tensile strengths of 95,000 lb. per sq. in. in chill-cast ingots and 85,000 when recast in sand molds, were usually obtained.

As to the hydrostatic test of castings, several confessedly imperfect castings of very large size, which had been rejected, the walls being about $1\frac{1}{4}$ in. thick in some cases and $1\frac{3}{4}$ in. in others, were submitted to hydrostatic tests beginning at about 100 lb. per sq. in. and increasing, not through minutes and hours, but days and weeks, up to several hundred pounds per square inch. Even these imperfect castings, after repairs by welding, successfully withstood these tests to an ultimate pressure of about 1000 lb. per sq. in., maintained for some time; and although

Mr. Flinn.

some incipient cracks were slightly opened, the openings were on the outside and the castings were tight under that enormous pressure, although designed for a working pressure of only about 130 lb. per sq. in. All my remarks about castings refer to manganese-bronze castings of the usual range of composition.

Mr. de Forest.

MR. A. V. DE FOREST.—Before we get away from the subject, I should like to ask whether we know enough about season cracking to be able to discuss ways of curing it. It seems to me that while we are talking about initial stresses and cracking, we should determine accurately whether season cracking and corrosion cracking are one and the same. To many people, they are the same thing. Mr. Lindsay's example of the cartridge cases looked exceedingly like a pure corrosion effect, but I do not think the case is as yet proved. A possible explanation of the rods Mr. Webster mentioned is that the sprung rod has an outside layer which is in compression and the unsprung rod an outside layer which is in tension. It is then easy to imagine that the corrosion effect on the material in tension will start cracks which cannot be started with a much higher compressive stress.

Also it would be interesting to know whether a simple protection against corrosion would not prevent the cracking entirely, even where there is a high internal stress.

Mr. Webster.

MR. WEBSTER.—The suggestion of Mr. de Forest that probably a protective coating would prevent season cracking has been tried out. Two or three years ago I drew some tubes very hard and left them in a condition which would render them exceedingly liable to season cracking. Several samples were very carefully painted; other samples were left with the natural finish. All the samples were submitted to the weather, and in a short time the unprotected tubes all cracked and none of the protected tubes did.

Mr. Lynch.

MR. T. D. LYNCH.—The different experiences that have been related remind me of a practical problem brought to my attention a few years ago on the subject of brittle phosphor-bronze wire. The cause and cure may be of interest at this time. This wire was drawn very hard in order to get a high tensile strength. The extremely hard drawing seemed to be more than the metal could stand for any length of time. Our tests on the material soon after it had been made gave very good results, but

Mr. Lynch. after the wire had been held in stock for a few weeks it was found to become extremely brittle, often breaking while in coils in stock due to the internal stresses. After considerable investigation it was found that the wire could be toughened by heat treatment, even the brittle wire being susceptible to toughening by immersing the wire in oil heated to 200° C. for a period of 2 hours. Before treatment the wire could easily be broken in the fingers. After treatment it was very tough and ductile, while the tensile strength was not in the least depreciated.

In another case a lot of tempered steel springs were breaking in service, to which a cooking treatment was given. This consisted in heating the springs in oil at 300° C. for a period of 2 hours. This treatment toughened the springs and no further trouble from brittleness was experienced.

The above and other experiences lead me to believe that castings and forgings can be improved to a very large degree by heating uniformly to about 300° C. for a sufficient length of time for the stresses to be relieved. The possibilities of removing stresses at relatively low temperatures are not generally appreciated and are worthy of consideration.

Mr. Price. MR. W. B. PRICE.—I think that while we are on the subject of so-called season and corrosive cracking, it should be understood, once for all, that season cracking is an erroneous term; it should be called corrosion cracking, for in my experience, and I think Mr. Bassett and Mr. Webster will bear me out, I have never seen a case of so-called season cracking that was not attended by corrosion of some sort.

Mr. Flinn. MR. FLINN.—Some experiments in the Board of Water Supply laboratory showed that brass, if stressed under the yield point or elastic limit and corroded, did not crack. Brass which was stressed beyond that limit, no matter whether by working stress or so-called initial stress, and corroded, did crack; in other words, corrosion seemed to be necessary with the stress to bring out the cracking, because even when subjected to excessive stress for a long time and protected, it did not crack.

HIGH-SILICA PORTLAND CEMENT.

By A. W. K. Billings.

SUMMARY.

In determining the suitability of given raw materials for the manufacture of Portland cement it is usual to insist that the proportions of the principal components—lime, silica, alumina and iron—lie within certain empirical limits. The possibility of producing satisfactory cements with materials whose composition lies outside of these limits is recognized by some authorities, but often the cement chemist relies upon the empirical formulas and arbitrarily rejects such materials without investigating whether they can be used and whether their use is economically justifiable.

A particular case (the construction of a large concrete dam in Spain) is cited in which experts pronounced the raw materials unsuitable because the proportion of silica was above the usual limits; special economic conditions, however, justified the installation of a mill for manufacturing cement from these materials. A modern cement mill was erected and a very satisfactory high-silica cement was produced. This cement is compared with commercial cements, and attention is called to the fact that high-silica cements are more reliable than average and high-alumina cements, and superior for uses which do not demand rapid hardening, but that they would frequently be rejected under a literal enforcement of present standard specifications. While high-silica cements will not come into the general market until purchasers are willing, under certain conditions, to pay more for them than for ordinary cements, there should be greater latitude in passing upon the suitability of such raw materials and in testing the resulting cements.

The conditions under which the manufacture of high-silica cement may be justifiable are discussed briefly.

HIGH-SILICA PORTLAND CEMENT.

By A. W. K. Billings.

In raw materials for Portland cement the proportions of magnesia, sulphur and other objectionable elements must be low and the proportions of lime, alumina, silica and iron must be such that the mix can be burned and ground properly and that the resulting cement will be sound and will attain the required strengths at stated periods.

In judging the suitability of given raw materials the relative proportions of the important components are usually required to fall within certain empirical limits. The utility of these different ratios or indices and the permissible limits of each have long been discussed on theoretical and on practical grounds, but the lack of any clear and generally accepted theory of the composition and behavior of cement has hindered the settlement of these questions. Though authorities admit the possibility of producing good cements which lie outside these limits, there is a tendency to employ certain of these rules rather blindly.

This note is therefore written from the standpoint of the engineer or user of cement, rather than the chemist or manufacturer. In the particular case which is described unusual economic conditions justified the manufacture of high-silica cement; its use in an important structure under very close inspection of a Spanish Government Commission required a careful study of its qualities; and the fact that the mill was later installed in a permanent location to manufacture cement of average composition under ordinary commercial conditions eliminates from the present discussion the natural bias of a manufacture in favor of his own product. None of the cement produced was sold, nor does the writer happen to know of any cement of nearly identical composition on the market. It is well, however, to call attention to the qualities and advantages of such cements and the conditions under which their manufacture and use may be economically justifiable.

Among the empirical rules frequently used in determining the suitability of given raw materials, may be mentioned the following:–

The "hydraulic index,"

$$\frac{SiO_2 + Al_2O_3}{CaO},$$

is generally required to lie between 0.50 and 0.42. The "lime ratio,"

$$\frac{CaO}{SiO_2 + Al_2O_3 + Fe_2O_3},$$

should lie, according to Meade, between 1.9 and 2.1, or, according to Michaelis between 1.8 and 2.2. The "index of activity,"

$$\frac{SiO_2}{Al_2O_3},$$

is usually required to lie between 2.5 and 5. Meade states that if this ratio lies between these limits, then the ratio

$$\frac{CaO}{SiO_2 - Fe_2O_3 - Al_2O_3} = 2.05.$$

The "silica modulus,"

$$\frac{SiO_2}{Al_2O_3 + Fe_2O_3},$$

is usually required to lie between 1.8 and 4.0.

Le Chatelier gives the following rules:

$$\frac{CaO + MgO}{SiO_2 + Al_2O_3} \leqq 3 \text{ (usually 2.5 to 2.7)}$$

$$\frac{CaO + MgO}{SiO_2 - Al_2O_3 - Fe_2O_3} \geqq 3 \text{ (usually 3.5 to 4.0)}$$

Newberry requires: $CaO = 2.8\,(SiO_2) + 1.1\,(Al_2O_3)$.

These rules are useful in routine work and under average conditions of commercial competition, but may be misleading when applied to unusual cases. For example, high cost of transportation, or the desirability of increased resistance to disintegration, may justify going against these rules.

Meade states that "high-silica cements are usually slow setting and of good tensile strength; they harden also slowly and usually show a progressive gain. Cements with an index of activity of more than 5 are usually very hard to burn; they are slow setting and also slow hardening. Their early strength is often lower, but ultimately they obtain as great strength as other cements. There are a number of high-silica cements on the American market which have great difficulty in meeting standard specifications as to seven-day strength."

The experience herein described confirms this statement. The fact, however, that most of the specialists consulted stated that the silica was too high to permit manufacture of cement, seems to indicate that insufficient attention was paid by them to the special economic conditions involved, and to the fact that a very high grade of cement can be made from such materials when desired.

In connection with extensive hydro-electric developments near Barcelona, Spain, climatic conditions made necessary a large reservoir for regulating the flow of the Noguera Pallaresa River. A suitable site was found near the town of Talarn in Lerida Province, and here a very high dam of gravity section was built using cyclopean concrete, the height above bedrock being 82 meters (270 ft.), or 104 meters (341 ft.) above the lowest point in the cutoff wall, the length of crest 207 meters (680 ft.), and the volume 270,800 cubic meters (355,000 cu. yd.) in the dam proper. The dam is the fourth or fifth highest in the world and much higher than any other in Europe; consequently very close attention was paid by the Spanish government engineers to the design, materials for construction and execution of the work.

The cost of transportation was an important factor, as the dam is situated in the foothills of the Pyrenees, one hundred and twenty miles from Barcelona and fifty-five miles from the nearest railroad; the construction of a temporary railroad was impracticable and all machinery, coal and other materials had to be hauled to the site by animal and mechanical traction over extremely bad roads. About 100,000 tons of cement were originally required,[1] only a small part of which could have been supplied by existing Spanish mills; imported cement was expensive and the added cost of transportation to the site and the delay in the work, if reliance had to be placed on these sources of supply, made it very desirable to manufacture a high-grade Portland cement at or near the site.

The dam is founded in a sandstone formation which is overlaid by an argillaceous limestone or "marl" about ten meters (33 ft.) in thickness, and this in turn by a layer of silicious limestone of similar thickness, from which the stone for the concrete was obtained. The surrounding country is composed of similar sedimentary deposits, all containing silica in considerable amount.

The materials were examined by an expert from London and duplicate samples were sent to another in New York. Both advised the company that the proportion of silica was too high to make satisfactory cement. An English manufacturing expert recommended a rather distant site where the proportion of silica was slightly lower, using a semi-wet process. After further careful study of the situation, the recommendation of an American cement mill engineer in the company's employ was approved, and it was decided to install the mill at the dam site and to use the materials immediately at hand. The economic conditions already indicated, and the conviction that a high silica cement was especially suited for this work, were considerations which outweighed the anticipated difficulty and extra cost of manufacture.

A modern two-rotary dry process plant, electrically equipped throughout, having a nominal capacity of 1500 barrels per day, was immediately ordered (August, 1912), installed as soon as received and put in operation just one year later. Ball and tube mills for both raw and finish grinding were adopted principally because of labor conditions. Waste heat utilization was adopted because fuel was expensive and a boiler plant was needed in any case for pushing the construction work while transmission lines, a smaller waterpower, and a steam plant on the railroad were being built. Motor drive was preferred because electric power would be available by the time the mill was installed, because later in its permanent location the mill would be a power customer of importance, and because the temporary location was a very cramped and irregular space on the cliff three hundred feet above the river and a satisfactory mechanical drive was difficult.

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[1] The outbreak of the present war, however, interrupted the work for nearly a year and caused a change in program, so that only 66,000 tons were manufactured before the mill was dismantled for removal to its permanent location in commercial work.

It may be mentioned that when the construction of the dam was completed this mill was sold to a cement manufacturing company, delivered at the dam site, for over 80 per cent of its initial cost delivered in Barcelona; the sale of electric power to the mill in its new location should bring in a revenue of $80,000 per year, and arrangements were included in the contract for the purchase of cement for future work at special prices. The financial as well as technical results were, therefore, deemed satisfactory.

TABLE I.—TYPICAL ANALYSES.

	Limestone.	Marl.	Cement.			
			Typical.	Range.	1914 Average.	1915 Average.
SiO_2, per cent	11.53[a]	33.92	26.28	25.00 - 28.00[a]	25.71	26.45
Al_2O_3, "	1.18	11.42	4.61	4.09 - 5.67	4.75	4.25
Fe_2O_3, "	0.55	3.56	2.01[b]	1.90 - 2.60	2.29	2.38
CaO, "	47.84	25.92	63.00	60.00 - 64.00	62.56	61.09
MgO, "	0.51	1.01	0.87	0.72 - 1.53	0.91	1.05
SO_3, "	.....	0.04	0.96	0.94 - 1.46	1.24	1.04
Loss on ignition, per cent	38.25	23.65	1.89	1.18 - 4.00[c]	2.23	3.66
	99.86	99.52	99.62	....	99.69	99.92
Hydraulic Index = (silica + alumina) ÷ lime			0.49	0.44 - 0.51	0.49	0.50
Lime Ratio = lime ÷ (silica + alumina + iron)			1.91	1.80 - 2.00	1.91	1.85
Index of Activity = silica ÷ alumina			5.70	5.00 - 7.00	5.41	6.23
Silica Modulus = silica ÷ (alumina + iron)			3.97	3.00 - 4.60	3.65	3.99

[a] Careful selection of limestone required to hold down the amount of silica in the finished cement.

[b] Some iron ore was added to facilitate clinkering; this component subject to independent control.

[c] Loss on ignition due to long period of weathering (up to two years) in open; proper grinding was difficult when loss was over 3.75 per cent, hence this was kept below 4.00 usually by drying or mixing with fresh clinker.

Typical analyses of the limestone, "marl," and cement are given in Table I.

During the early part of the work the Spanish government engineers had a series of tests made at different laboratories comparing random samples of this and of two other high-grade Spanish cements, and of one English cement which was in use on other work. These results and also the average of the mill tests for the Ebro (high-silica) cement are given in Table II and in Fig. 1.

As was to be expected, the high-silica cement ("Ebro" brand) set within the usual time, but hardened very slowly; it was uniformly sound and the autoclave test always gave satisfactory results. The strength at one, 7 and 28 days was frequently less than that required by standard specifications and the literal enforcement of these would have resulted in the rejection of a large part of the output of the mill. The strength at later periods was greater than that of first-class cements of usual composition, and the steady gain in long-time tests, which was especially evident in concrete made with this cement, was one of the most convincing indications of its reliability and sustained high strength. The Spanish government engineers, at first very skeptical, became fully convinced of the high quality of this cement and heartily approved the use of it.

A very extensive series of concrete tests was carried on during the execution of the work, but no attempt will be made to discuss them here as the results are of somewhat special application. No natural sand whatever was used in the construction of the dam, the concrete being made from a carefully graded mixture of crushed limestone ranging from 3-in. down to the finest dust. The density, impermeability and strength were much higher than with ordinary sand and stone. The density averaged about 2.42, that of the limestone being 2.68. Using 230 kg. per cu. m. (1.03 bbl. per cu. yd.) of cement, compression tests on 15-cm. (6-in.) cubes taken daily from the mixers, stored in water for 84 days and then in air, gave results as follows:

	7 DA.	28 DA.	84 DA.	6 MO.	1 YR.
Compressive Strength, lb. per sq. in...	745	1390	2200	2910	3290

(The increase in strength after 3 months is partly due to drying out of the cubes.)

Cubes cut from the dam gave somewhat higher results, in some cases attaining 5700 to 6200 lb. per sq. in. The government

TABLE II.—COMPARISONS OF VARIOUS CEMENTS.

Properties.	Spanish Portland Cements.				English Cement.		"Ebro"[1] (High-Silica) Cement.		Average of "Ebro."	
	"Leon."		"Asland."		"Pyramid."		Random Sample.		Mill Tests.	
	Neat.	1 : 3	Neat.	1 : 3	Neat.	1 : 3	Neat.	1 : 3	Neat.	1 : 3
SPECIFIC GRAVITY	3.077	...	3.147	...	3.122	...	3.139	...	3.161	...
FINENESS:										
Passing No. 100, per cent	97.5	...	95.9	...	98.5	...	97.3	...	97.2	...
" No. 200, "	80.0	...	77.6	...	85.8	...	77.4	...	79.5	...
SETTING TIME (VICAT), hr. and min.:										
Initial	3 : 25	...	3 : 05	...	5 : 10	...	5 : 40	...	3 : 18	...
Final	7 : 30	...	5 : 30	...	8 : 35	...	9 : 55	...	6 : 23	...
Le Chatelier Test, mm	1.0	...	17.0	...	1.5	...	2.5	...	1.3	...
WATER, per cent	26.0	8.85	25.0	8.66	27.5	9.09	25.5	8.75	...	...
TENSILE STRENGTH, lb. per sq. in.:										
1 day	331	...	289	...	105	...	125	...	206	...
7 days	705	323	679	265	570	238	481	196	481	200
28 days	819	406	757	323	673	375	651	303	745	330
84 days	782	481	717	435	734	440	865	391	857	434
6 months	722	484	771	397	772	448	1009	433	889	464
1 year	761	457	826	398	754	432	999	474	906	488
Autoclave	...	...	...	...	...	...	...	...	772	339
CHEMICAL ANALYSIS:										
SiO_2, per cent	22.36	...	21.74	...	22.30	...	26.28	...	25.71	...
Al_2O_3, "	6.87	...	6.98	...	7.15	...	4.61	...	4.75	...
Fe_2O_3, "	2.05	...	2.72	...	2.65	...	2.01	...	2.29	...
CaO, "	62.85	...	62.35	...	62.65	...	63.00	...	62.56	...
MgO, "	1.66	...	2.75	...	1.22	...	0.87	...	0.91	...
SO_3 "	1.46	...	1.64	...	0.92	...	0.96	...	1.24	...
Loss on Ignition, per cent	2.44	...	1.42	...	2.03	...	1.89	...	2.23	...
Hydraulic Index	0.47	...	0.46	...	0.47	...	0.49	...	0.49	...
Lime Ratio	2.01	...	1.98	...	1.95	...	1.91	...	1.91	...
Index of Activity	3.25	...	3.11	...	3.12	...	5.70	...	5.41	...
Silica Modulus	2.50	...	2.24	...	2.27	...	3.97	...	3.65	...

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[1] The random sample was taken from near the top surface of a bin where the cement was coarser than the average, and in consequence the neat tests are somewhat higher and the mortar tests somewhat lower than usual.

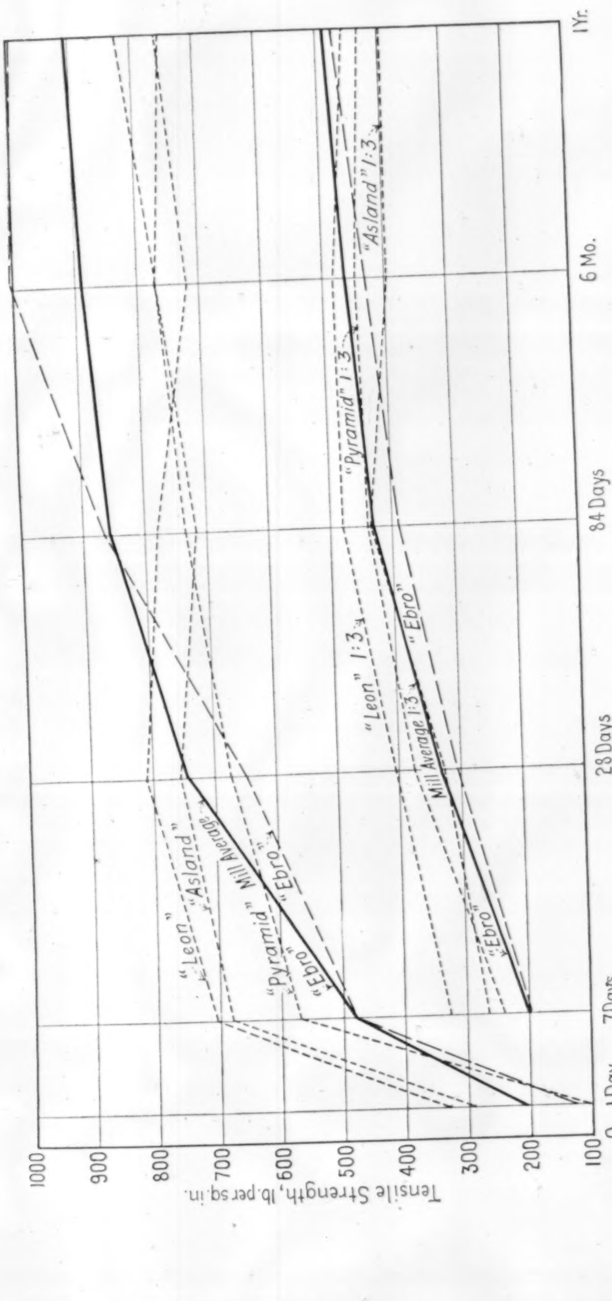

FIG. 1.—Graphical Comparisons of Various Cements.

required a minimum strength of 2135 lb. per sq. in., and the greatest compressive stress in the dam, calculated by Levy's theory, was about 285 lb. per sq. in. The satisfactory results in the concrete work are due in part to the intelligent use of artificially graded, crushed mixtures and in part to the use of high-silica cement.

The development of internal heat in the mass of concrete from the chemical reactions involved in the setting of the cement was much less marked than with cements containing more alumina, and consequently the subsequent contraction of the dam has been imperceptible. Usually in a mass of concrete of these dimensions the interior attains a temperature 20 or 30° C. higher than the final average temperature, and when, after a few years, the latter is reached, considerable contraction is evident, amounting to one or two parts in ten thousand in linear dimensions. In the present case no such contraction is apparent, nor was there at any time evidence of appreciable rise of temperature due to chemical reactions. For this reason it is necessary, in working with high-silica cement, to pay special attention to protection from freezing in winter weather, as hardening is retarded and freezing of exposed surfaces occurs more rapidly.

In the manufacture of high-silica cement the temperature required in the kiln for the clinkering of the mix is considerably higher than for average cements, resulting in increased fuel consumption and cost of maintenance of the kiln lining. Considerable difficulty was experienced at the start due to several causes which were remedied, generally speaking, simultaneously. Insufficient care in selecting the materials, lack of experience of the native burners, improper air supply for the pulverized coal, the use of a local firebrick, and the need of more iron in the mixture were remedied without serious trouble and the product was uniformly satisfactory thereafter, although the percentage of silica had to be watched closely at all times. It was found advantageous to add from one to two per cent of low-grade iron ore to the mix to increase its fusibility.

It is not possible to calculate accurately the increased cost of manufacture of this cement, because no materials for a normal mix were available for comparison. It is estimated, however, that the increased coal consumption amounts to about six per cent of the weight of the cement. The lining maintenance is not excessive, but depends greatly on the skill of the burner. If the proportion of iron can be controlled, by adding iron ore or otherwise, this difficulty is not important; otherwise a practical limit is quickly reached, on account of the rapid increase in fuel consumption.

No appreciable difference from ordinary conditions in the ease of grinding the clinker was noted. The clinker required only a brief period of storage, if any, to ensure soundness, but construction conditions, and especially the interruption of the work for one year by the present war, necessitated storing a large amount of clinker in the open for one or two years. This weathering increased the tendency to slowness in hardening, especially when the loss on ignition (due principally to combined moisture) was allowed to exceed 3 per cent. When this reached 4 per cent, the amount of steam generated in the tube mills prevented proper grinding.

The usual tests for constancy of volume and soundness under normal and accelerated tests gave uniformly satisfactory results, although, as with any cement, coarse grinding of the raw materials gave bad results in the boiling test with unseasoned clinker. The autoclave test was always favorable and gave results which corresponded to normal tests at about 28 days. While we believe that the autoclave test favors a high-silica cement, we were unable to determine any relation between the results of these accelerated tests and the ultimate strength of the cement and mortar mixtures under normal conditions. We do believe, however, that a cement which passes the autoclave test is one which will reach a high ultimate strength and will prove satisfactory especially in mass concrete work.

High-silica cement resists disintegration from exposure to the action of gypsum, alkali and sea-water, where high-alumina cements suffer serious damage. No tests were made in sea-water, but a large irrigation system (the Canal of Aragon and Catalonia, operated by the Spanish government) had experienced much difficulty in the maintenance of its tunnel and canal linings in gypsum-bearing formations, and at the request of the engineers in charge a quantity of the high-silica cement was sent to be tested in comparison with a standard Spanish cement ("Asland") and a German iron cement ("Erzzement").

TABLE III.—EFFECT OF GYPSUM AND OTHER SULFATES ON PORTLAND CEMENT.

(FROM TESTS MADE AT THE LABORATORY OF THE CANAL OF ARAGON AND CATALONIA, MONZON, SPAIN).

Results after 175 Days.		High-Silica Cement. (Ebro)			High-Iron Cement. (Erzzement)			Average Portland Cement (Asland).		
		Tensile Strength,		Pats.	Tensile Strength,		Pats.	Tensile Strength,		Pats.
		kg. per sq. cm.	lb. per sq. in.		kg. per sq. cm.	lb. per sq. in.		kg. per sq. cm.	lb. per sq. in.	
Immersed in pure water............................	Neat....	45.33	644	O. K.	.....	...	....	.....	...	....
	1 : 2½...	33.17	471	O. K.	.....	...	....	.....	...	....
	1 : 4....	24.00	341	O. K.	.....	...	....	.....	...	....
	1 : 5....	.....	...	....	.....	...	....	.....	...	....
Immersed in springwater, containing: Calcium sulfate, 1.66 grams per liter Magnesium sulfate, 1.52 " " " Magnesium chloride, 0.19 " " " Sodium sulfate, 2.12 " " "	Neat....	43.67	621	O. K.	54.80	779	O. K.	50.00	711	O. K.
	1 : 2½...	33.17	471	O. K.	31.75	451	O. K.	17.07	243	O. K.
	1 : 4....	23.67	337	O. K.	.....	...	....	.....	...	....
	1 : 5....	.....	...	....	21.27	303	O. K.	*	*	*
Embedded in pulverized gypsum and wetted down twice daily..	Neat....	40.33	573	O. K.	41.00	583	O. K.	55.00	781	O. K.
	1 : 2½...	35.33	502	O. K.	21.70	308	**	24.50	348	O. K.
	1 : 4....	23.00	327	O. K.	.....	...	....	.....	...	....
	1 : 5....	.....	...	....	8.20	117	**	5.00	71	*
Embedded in mixture one-half gypsum and one-half clay and wetted down twice daily......................	Neat....	43.00	611	O. K.	55.00	781	O. K.	58.70	835	O. K.
	1 : 2½...	33.50	476	O. K.	25.50	362	**	26.00	370	**
	1 : 4....	23.60	336	O. K.	.....	...	....	.....	...	....
	1 : 5....	.....	...	....	13.80	196	**	7.00	100	*
Embedded in mixture one-third gypsum, one-third clay, one-third sand and wetted down twice daily.......	Neat....	44.83	637	O. K.	43.80	623	O. K.	58.00	825	O. K.
	1 : 2½...	34.00	483	O. K.	22.40	318	***	26.00	370	O. K.
	1 : 4....	23.00	327	O. K.	.....	...	....	.....	...	....
	1 : 5....	.....	...	....	10.50	149	O. K.	4.20	60	*

* Disintegrated.

** Edges disintegrated.

*** Broke in handling due to disintegration.

TABLE III.—EFFECT OF GYPSUM AND OTHER SULFATES ON PORTLAND CEMENT—(*Continued*).

Results after 175 Days	High-Silica Cement. (Ebro)			High-Iron Cement. (Erzzement)			Average Portland Cement (Asland).		
	Tensile Strength,		Pats.	Tensile Strength,		Pats.	Tensile Strength,		Pats.
	kg. per sq. cm.	lb. per sq. in		kg. per sq. cm.	lb. per sq. in.		kg. per sq. cm.	lb .per sq. n.	
Immersed in 28-per-cent solution of sodium sulfate..... Neat....	52.50	746	O. K.	.....	...	....	.....	...	....
1 : 2½...	34.00	483	*	.....	...	....	.....	...	....
1 : 4...	***	***	*	.....	...	....	.....	...	....
CHEMICAL ANALYSIS:									
SiO_2, per cent		26.32			19.47			20.65	
Al_2O_3, "		3.95			3.47			7.45	
Fe_2O_3, "		2.55			9.44			3.12	
CaO, "		62.32			64.39			62.25	
MgO, "		1.13			0.65			2.22	
SO_3, "		1.09			2.04			1.50	
Loss on ignition, per cent		2.69						....	
Hydraulic Index		0.49			0.47			0.45	
Lime Ratio		1.90			1.99			1.99	
Index of Activity		6.66			5.60			2.77	
Silica Modulus		4.05			1.51			1.95	

* Disintegrated.
*** Broke in handling due to disintegration.

Briquettes for tensile tests and pats were made and exposed for 175 days to the action of saline waters and mixtures. The summary of part of the tests given in Table III has been combined with previously published results and shows that the high-silica cement resists best, and that the only case of disintegration with this cement was with a 1 : 4 mortar immersed 175 days in a 28-per-cent solution of sulfate of sodium. The neat and 1 : 2½ mortar briquettes exposed to the same solution showed no loss of strength whatever. The results were, in fact, slightly higher than those for the check tests in pure water.

It is regretted that at the outset a thorough test in sea-water with this cement was not started, but such an investigation was entirely beyond the scope of any contemplated work. The distance from the coast forbade attempting to use this cement in port works, and when the mill was later moved to its permanent location, it was intended to manufacture a normal cement in order to meet commercial competition.

However, the effect of gypsum and other sulfates and alkalies on cement is very similar to that of sea-water. It is the writer's conviction that, in selecting cement for work in sea-water, or where exposed to sulfates and alkaline salts, preference should always be given to the one containing the most silica and the least alumina. The percentage of iron would thus be considered a secondary matter, and of more importance to the manufacturer in controlling the fusibility of the clinker than to the user in aiding to resist disintegration of the concrete.

More attention should be paid to the statement of the chemical analysis in connection with cement and concrete tests. The lack of this information seriously diminishes the value of the majority of published tests, and the omission of long-time results in many cases still further limits the deductions which can be made. It is desirable that the analysis should always be given, and eventually on many orders of importance the limits of variation of the principal components will, it is believed, be specified.

In résumé, if the customary empirical rules show that the raw materials will produce a normal cement, no further investigation of this point is essential; if, however, they indicate the possibility of producing a cement of somewhat abnormal composition, a careful study of the qualities of the resulting cement, the purpose for which intended, and the attendant economic conditions, is essential before a decision is reached. A high-silica cement requires in manufacture closer attention, more fuel and more frequent renewal of the kiln linings; an increase in the proportion of iron aids materially in controlling these difficulties, but high silica necessarily means increased cost of manufacture. In purely commercial competition with nearby mills in an open market such a cement would be at a considerable disadvantage. For ordinary reinforced-concrete work, where quick hardening permits rapid construction and reduced cost of forms, a normal cement, or even one high in alumina, will be preferred. For dams, mass concrete work, reinforced work where no considerable stress is applied for several weeks or months, and especially in all cases where resistance to disintegration and high ultimate strength are important, a high-silica cement is preferable.

The user of cement for such work should accept low early strengths, if good strength at later periods is assured, and may therefore permit under intelligent inspection a reduction below standard specifications in the requirements for routine acceptance tests. For work in sea-water, high-silica cement is worth more to the user and should command a higher price, in proportion to the increased cost of manufacture. More attention to the chemical composition of cements on the part of the engineer and user is desirable.

DISCUSSION.

Mr. Newberry. MR. S. B. NEWBERRY (*presented in written form and read by the Secretary*).—I have read with much interest the preprint of the paper by Mr. Billings. The suggestions and information he gives in reference to the behavior of cements high in silica are important and useful. Referring, however, to the low strength at short periods which the high-silica cement of the "Ebro" brand shows, I would point out that the typical analysis of this cement shows it to be decidedly low in lime. If its composition had been normal, with perhaps 65 to 66 per cent of lime, it would certainly have shown practically normal strength at short periods.

In calculating a normal composition for Portland cement there is, I am satisfied, no formula which properly reveals the character of the cement except that suggested by Le Chatelier and modified and simplified by myself, as quoted in Mr. Billings' paper. This formula: $CaO = 2.8\ (SiO_2) + 1.1\ (Al_2O_3)$, may be transposed to read,

$$\frac{CaO - 1.1\ (Al_2O_3)}{SiO_2} = 2.8.$$

This must be understood, however, to represent practically the maximum amount of lime consistent with good hot tests for constancy of volume. I have proposed to call this figure the "lime factor." In properly proportioned cement raw materials this may be as high as 2.8, but for practical purposes it is better to hold it at about 2.7. Owing to the effect of the ash of the fuel used in burning, the lime factor of finished cement is usually considerably lower, for example, 2.5 to 2.6.

I find by calculation from the typical analysis of Ebro cement given in Table I of the paper, that the lime factor of this cement is only 2.22. This is decidedly too low for prompt hardening and shows the cement to be distinctly overclayed. It is well known that overclayed cements, while they may be quick setting, are always slow in hardening, showing low strengths

Mr. Newberry.

at short periods, although they may show very superior strength at one year or longer. It is probable that this low-lime composition was adopted in the case of the Ebro cement to prevent excessive fuel cost in burning. This was undoubtedly accomplished at the expense of prompt hardening. The company with which I am connected has for many years made cement at two of its plants which shows a silica ratio of 3.5 to 3.6, or not much below that of the Ebro cement, which shows a silica ratio of 3.97. We have in fact successfully made cement of as high silica ratio as 4.5 at considerably increased expense for fuel, but have obtained with this cement perfectly normal tests at 7 and 28 days, and longer periods.

APPARENT SPECIFIC GRAVITY OF NON-HOMOGENEOUS FINE AGGREGATES.

By A. S. Rea.

SUMMARY.

The object of this investigation was to devise a method by which the "apparent" specific gravity of sand or other fine aggregates can be accurately determined.

In general the "true" specific gravity of materials of this class does not offer any indication of the value or suitability of such materials for the purposes they are intended.

On the other hand the "apparent" specific gravity as determined by the usual displacement method is extremely inaccurate for aggregates containing a considerable percentage of relatively porous particles or grains.

The method herein described for the determination of the "apparent" specific gravity of fine aggregates makes provision for the porosity of the grains, and hence gives results which are more indicative of the properties of this class of aggregate than is given by the true specific gravity determination.

APPARENT SPECIFIC GRAVITY OF NON-HOMOGENEOUS FINE AGGREGATES.

By A. S. Rea.

The specific gravity or relative weight of such materials as rock, slag, gravel, sand, and similar materials used in road and building construction is generally considered of sufficient importance to justify the inclusion of this determination in the routine tests of such aggregates.

The specific gravity of such materials may be expressed as the "true specific gravity" or as the "apparent specific gravity." The first of these is applied to the specific gravity of the substance of which the mass is composed, while the second is applied to the specific gravity of the mass including the air spaces or voids.

From the standpoint of their use as aggregates the apparent specific gravity determination of such materials is the more important for the reason that the true specific gravity determination will not in general give any indication of the value or suitability of such materials for construction purposes.

Until about four years ago one of the most common methods of determining the specific gravity of such materials as rock was the so-called displacement method by which the specimen is first weighed in air, then in water and from these results the specific gravity calculated. It was found, however, that this method was not accurate for porous materials due to the fact that no provision was made for the absorption of water by the specimens while the determination was being made. At the present time there are a number of methods in use by which the apparent specific gravity of rock and similar materials may be satisfactorily determined. Practically all of these methods make provision for the porosity of the material by immersing the specimens in water until no more water is taken up after which the specimens are surface dried and the volume determined by some displacement method.

It is evident that such a method is not practical for fine aggregates, such as sand, for the reason that it involves, after the immersion of the specimens in water, the subsequent wiping of the surfaces of the individual pieces to remove excess of water. This of course cannot be done satisfactorily for a large number of small grains below a quarter of an inch in size.

It is likewise evident that if the sand contains an appreciable percentage of relatively porous grains, such as sandstone, shale, or limestone fragments that it will not be possible to obtain an accurate determination of the apparent specific gravity of the material by the usual displacement method used for such materials in which the volume is determined by the displacement of water or other liquid in some form of specific gravity flask or graduated cylinder.

Recognizing the desirability of having a method of determining the apparent specific gravity of non-homogeneous fine aggregates in such a manner that the porosity of the grains would be provided for, led the writer to make some investigations and experiments with this object in view. The experiments conducted covered a considerable number of different methods and procedures of which it is unnecessary for the purpose of this paper to describe or discuss. The method which was the most satisfactory from the standpoint of both accuracy and convenience and has been adopted in the Testing Laboratory of the Ohio State Highway Department as a standard test is described as follows:

Method of Determining the Apparent Specific Gravity of Fine Aggregates.—Five hundred grams of the thoroughly dried sample are placed in a 500-cc. graduated cylinder provided with a glass stopper. Approximately 20 cc. of kerosene or a sufficient quantity to saturate the grains and allow for a slight excess is then poured into the cylinder. The stopper is inserted and the cylinder is vigorously shaken until all of the sand grains appear to be coated with kerosene. A measured quantity of water, usually 250 cc., is then introduced into the cylinder. The cylinder is then thoroughly shaken to bring the excess of kerosene to the surface of the water. The two liquids, kerosene and water, being non-miscible and of different specific gravity, a clean line of demarcation will form between them and the reading of the volume of water can be readily obtained. From the dry weight of the aggregate and the volume of the liquid displaced, the "apparent" specific gravity of the aggregate can be calculated.

In this method the grains of sand retain the kerosene in their pores by capillary attraction, so that in displacement the volume is not that of the substance composing the grains, but the volume of the grains themselves. Any excess of kerosene beyond that necessary to fill the pores rises to the surface of the water.

TABLE I.—RESULTS OF SPECIFIC GRAVITY DETERMINATIONS.

Sample.	By Usual Displacement Method.	By Method Described.	Variation.
Standard Ottawa Sand	2.65	2.65	0.00
Limestone Screenings	2.63	2.55	0.08
Sandstone	2.47	2.41	0.06
Sand G-1312	2.63	2.52	0.11
" " 1319	2.60	2.50	0.10
" " 1320	2.59	2.46	0.12
" " 1321	2.63	2.50	0.13
" " 1325	2.60	2.58	0.02
" " 1328	2.57	2.54	0.03
" " 1329	2.63	2.62	0.01
" " 1331	2.72	2.66	0.06
" " 1333	2.63	2.60	0.03
" " 1336	2.65	2.63	0.02
" " 1344	2.53	2.48	0.05

In order to determine whether an appreciable film of kerosene adheres to the surface of the grains, which would thus result in an increased volume of liquid displaced with a consequent lowering of the specific gravity, determinations by this method were made on standard Ottawa sand. This sand is composed of practically pure quartz grains, hence its absorption need not be considered. The results showed this film to be so slight as not to appreciably affect the results.

To determine on the other hand whether the grains will completely retain the kerosene in the cavities or pores during the process of shaking the sand in water, a series of determinations was made on samples of crushed stone and slag of predetermined specific gravity. The results here showed that except in cases where the aggregate contained unusually large cavities or pores, as in some very porous slags, this method was entirely satisfactory.

In making the specific gravity determination by this method, it is essential for accurate results that the aggregate be clean. If the sand contains an appreciable percentage of silt it should be washed, otherwise the silt will be held in suspension in the kerosene at the top of the water, thus introducing a slight error as well as making the reading more difficult. In order to show the difference in the results obtained by this method as compared to the old displacement method, the results obtained in one series of tests are given in Table I.

From the figures given in the table, it will be noted that the difference in the specific gravity by the two methods ranges from zero with the Ottawa quartz sand which has a negligible absorption to 0.13 in the case of sand G-1321 which is a very coarse-grained sand consisting almost entirely of rounded sandstone fragments. Between these limits the specific gravity of the sands show variations depending upon the extent of the porous grains present in the sand.

PROPERTIES OF CEMENT-LIME-SAND MORTARS.

BY WARREN E. EMLEY.

SUMMARY.

In this paper an attempt is made to show some of the more important properties of a cement-lime-sand mortar, especially the manner in which these properties are affected by changes in the proportions of the ingredients.

A careful study of the results should enable one to interpret them to apply to the particular conditions with which he has to deal.

It is hoped that the method of expressing the results will prove of value, as being more comprehensive than those methods which are generally used.

PROPERTIES OF CEMENT-LIME-SAND MORTARS.

By Warren E. Emley.

It is customary to add more or less lime to cement mortars, in order to improve their working qualities. For this purpose, hydrated lime is so much more convenient than quicklime that it has practically supplanted the latter. The convenience of the hydrate lies in the fact that it is a powder, like cement, and can, therefore, be weighed out and added in definite proportions. The proper amount of lime to use has been the subject of a great deal of discussion. It is hoped that the data contained in this paper will give some information concerning the properties of cement mortars containing hydrated lime.

The quantity of lime used is generally stated in terms of percentage of the cement, the percentage being taken either by weight or by volume. The lime may be added to the cement mortar, or it may replace an equal weight or volume of cement. There are thus four methods of stating the proportion of lime:

Method A...Percentage of lime, by weight of cement, added;
" B...Percentage of lime, by weight of cement, substituted;
" C...Percentage of lime, by volume of cement, added;
" D...Percentage of lime, by volume of cement, substituted.

In order to represent a given mortar on the familiar triaxial diagram, it is necessary to express the quantities of all three materials as percentages by weight of the total weight. This may be done graphically as follows: In Fig. 1, the perpendicular ordinates from any point in the triangle to the sides *AB*, *BC* and *CA* represent respectively the percentages of sand, cement and lime in the mortar represented by that point. Therefore, the apices, *C*, *A* and *B*, represent respectively

100 per cent of sand, of cement and of lime. Suppose that it is desired to represent in the diagram a 1 : 3 cement-sand mortar, to which has been added an amount of lime equal to 20 per cent by weight of the cement; that is, a mortar proportioned by method A. The proportion of cement to sand remains 1 : 3, while the proportion of lime to cement is 20 : 100. Therefore, divide *CA* into *CD* and *DA* in the proportion of 1 : 3, and draw *BD*; then any point on *BD* represents a mortar in which the ratio of cement to sand is 1 : 3. Similarly, by drawing *CE* so that *E* divides *AB* into *AE* and *EB* in the proportion of 20 : 100, any point on *CE* represents a mortar in which the lime and cement are in that ratio. Therefore *P*, the intersection of *BD* and *CE*, represents the mortar to be plotted, the ordinates from *P* to *AB*, *BC* and *CA* being respectively the percentages, by weight of the total, of sand, cement and lime.

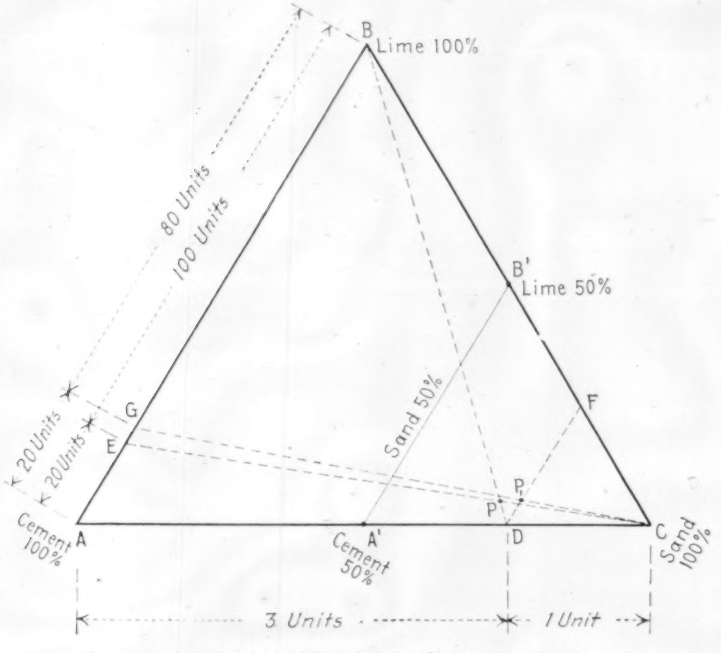
B
Lime 100%
80 Units
100 Units
B'
Lime 50%
Sand 50%
G
E
F
20 Units
20 Units
P₁
P
Cement 100%
A
A'
Cement 50%
D
C
Sand 100%
3 Units
1 Unit

FIG. 1.—Application of Tri-Axial Diagram to Plotting of Cement-Lime-Sand Mortars.

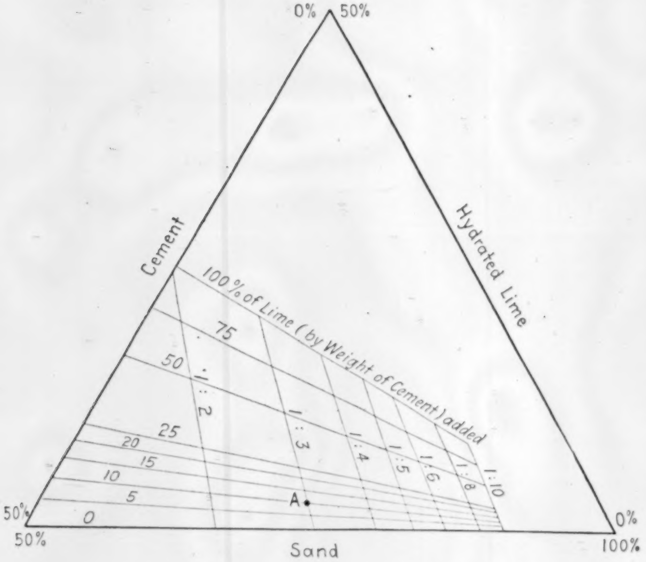
0%
50%
Cement
Hydrated Lime
100% of Lime (by Weight of Cement) added
75
50
25
20
15
10
5
0
1:2
1:3
1:4
1:5
1:6
1:8
1:10
A
50%
50%
0%
100%
Sand

FIG. 2.—Tri-Axial Diagram Locating Mortars Proportioned by Method A.

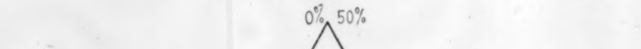
0%
50%
Cement
Hydrated Lime
100% of Lime (by Weight of Cement) Substituted
75
50
25
20
15
10
5
0
1:2
1:3
1:4
1:5
1:6
1:8
1:10
50%
50%
0%
100%
Sand

FIG. 3.—Tri-Axial Diagram Locating Mortars Proportioned by Method B.

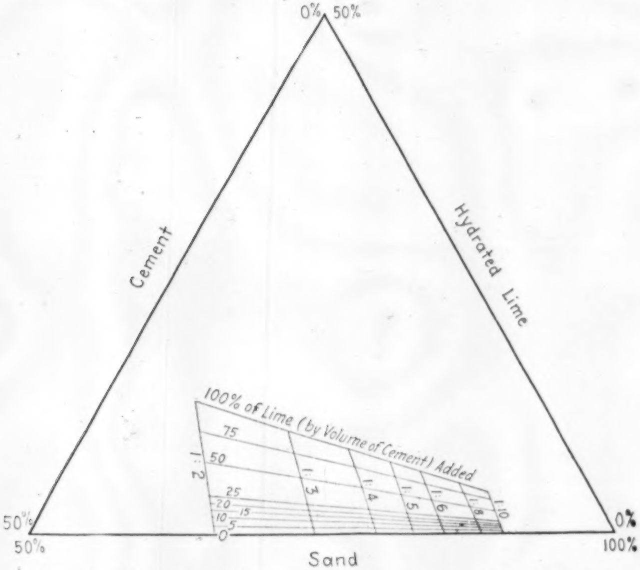
0%
50%
Cement
Hydrated Lime
100% of Lime (by Volume of Cement) Added
75
50
25
20
15
10
5
0
1:2
1:3
1:4
1:5
1:6
1:8
1:10
50%
50%
0%
100%
Sand

FIG. 4.—Tri-Axial Diagram Locating Mortars Proportioned by Method C.

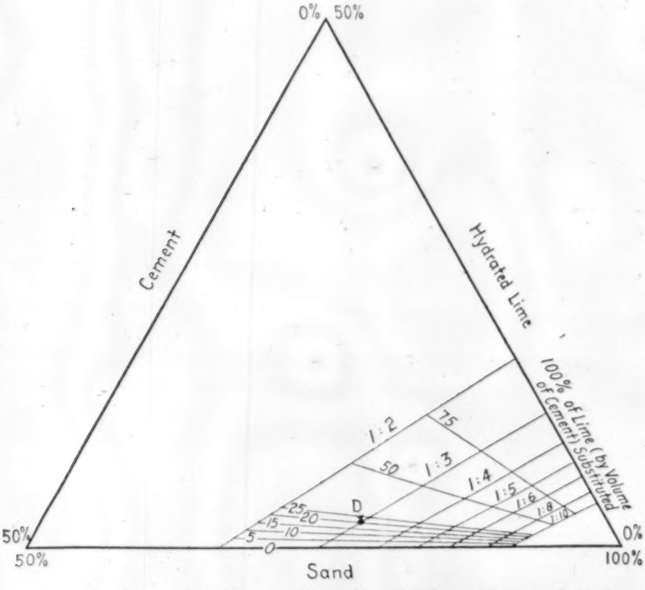
0%
50%
Cement
Hydrated Lime
100% of Lime (by Volume of Cement) Substituted
75
50
25
20
15
10
5
0
D
1:2
1:3
1:4
1:5
1:6
1:8
1:10
50%
50%
0%
100%
Sand

FIG 5.—Tri-Axial Diagram Locating Mortars Proportioned by Method D.

In proportioning by method B, the percentage of sand by weight of the total is constant for any percentage of lime. Thus in a 1 : 3 cement-sand mortar in which 20 per cent by weight of the cement is replaced by an equal weight of lime, the proportion of sand remains, as before, 75 per cent of the total, while the lime and cement are in the ratio of 20 : 80. This mortar will therefore be represented by P_1.

Mortars proportioned by methods C and D may be plotted in a similar way; cement has been assumed to weigh 100 lb. per cu. ft., and hydrated lime 40 lb. per cu. ft., in accordance with the usual practice.

Figs. 2 to 5, inclusive, have been plotted in this way for various mortars and percentages of lime, proportioned by methods A to D respectively. Since there is usually at least 50 per cent of sand in a cement-lime-sand mortar, only that part of the tri-axial diagram corresponding to $A'\ B'\ C$, Fig. 1, has been shown in Figs. 2 to 5.

These figures may best be used by preparing tracings of them, which can be laid directly over other diagrams for comparison, as explained below.

Forty-five mortars of varying proportions were each tested to determine the following properties:

1. Percentage of water for normal consistency;
2. Soundness;
3. Time of set;
4. Yield;
5. Cost per cubic foot of mortar;
6. Compressive strength at 7 days, in air;
7. “ “ “ 6 months, in air;
8. “ “ “ 6 “ “ water;
9. Tensile “ “ 6 “ “ air;
10. “ “ “ 6 “ “ water.

The mortars tested are represented by the circles in Figs. 6 to 15. After marking at each circle the test value obtained for that mortar, which in all cases is the average of three tests, points of equal value were obtained by interpolation and connected by lines as shown in the figures.

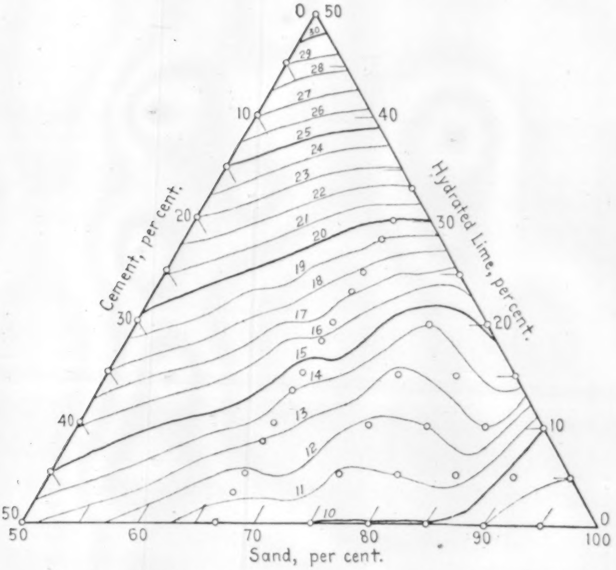

FIG. 6.—Normal Consistency; Percentage of Water, by Weight of Dry Material, required for Normal Consistency by the Ball Method.

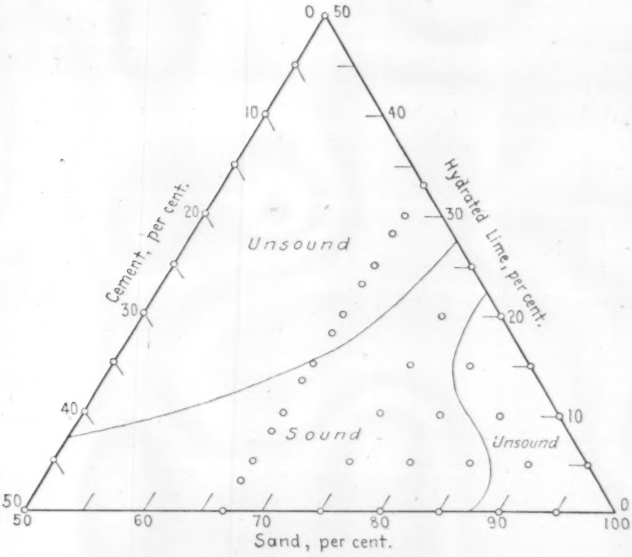

FIG. 7.—Soundness; Tested as prescribed by the United States Government Cement Specifications.

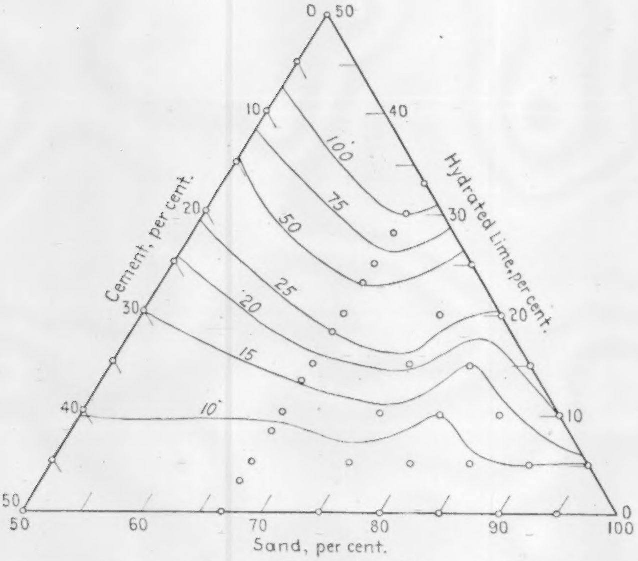

FIG. 8.—Time of Set; Final Set in Hours (Normal Consistency)

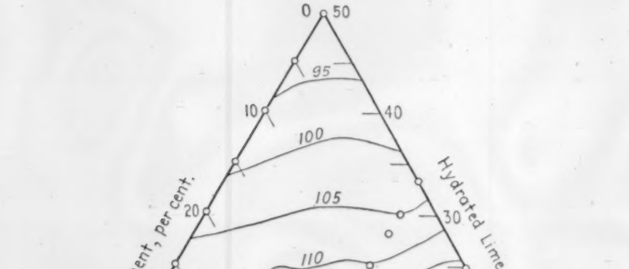

FIG. 9.—Yield; Number of Pounds of Dry Material required to Produce One Cubic Foot of Mortar of Normal Consistency.

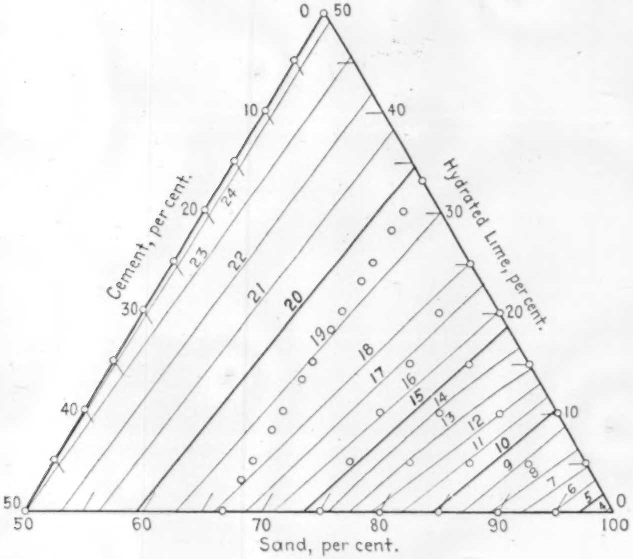

FIG. 10.—Cost; Cents per Cubic Foot at Normal Consistency. Hydrated Lime, $10 per ton; Cement, $1.25 per bbl.; Sand, $0.80 per ton.

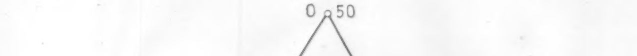

FIG. 11.—Compressive Strength, in Pounds per Square Inch Specimens, 7 Days Old, Stored in Air (Normal Consistency)

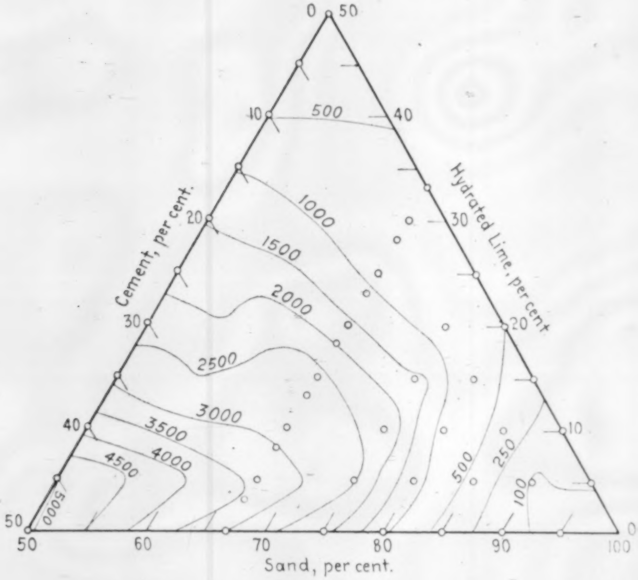

FIG. 12.—Compressive Strength, in Pounds per Square Inch. Specimens, 6 Months Old, Stored in Air (Normal Consistency).

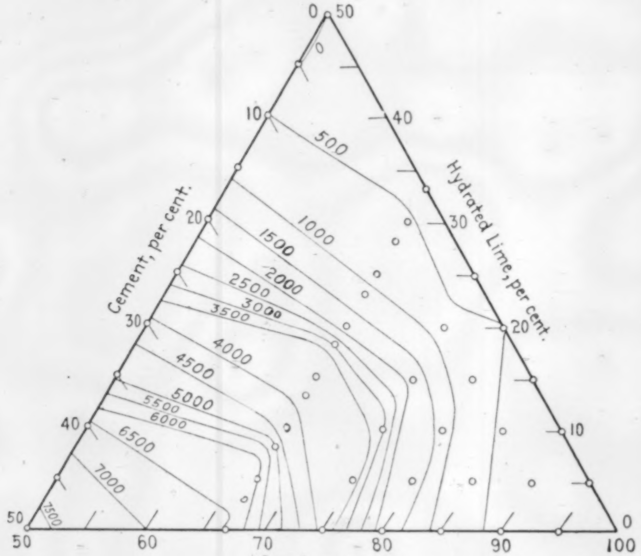

FIG. 13.—Compressive Strength, in Pounds per Square Inch. Specimens, 6 Months Old, Stored in Water (Normal Consistency).

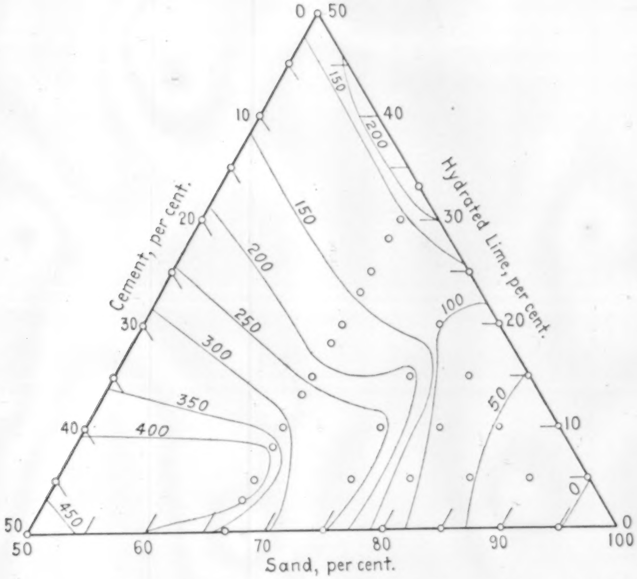

FIG. 14.—Tensile Strength, in Pounds per Square Inch. Specimens, 6 Months Old, Stored in Air (Normal Consistency).

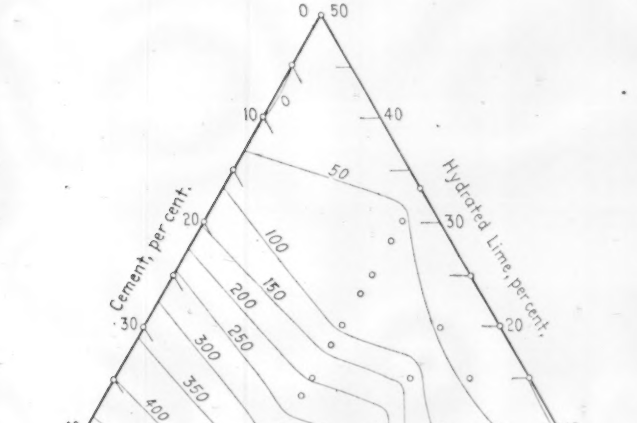

FIG. 15.—Tensile Strength, in Pounds per Square Inch. Specimens, 6 Month Old, Stored in Water (Normal Consistency).

Example of Use of Charts.

Suppose it is desired to compare a 1 : 3 mortar with 10 per cent of lime, by weight of cement, added, with a similar mortar in which 20 per cent of the volume of the cement has been replaced by an equal volume of lime. The first mortar will be found on Fig. 2, at the point marked *A*. The second is on Fig. 5, at the point marked *D*. Having marked these points on the tracings of the charts, these tracings can be placed over each of Figs. 6 to 15 successively, and the properties of the mortars obtained by interpolation. The results are as follows:

Fig. No.	Property.	Mortar A.	Mortar D.
6	Normal Consistency; percentage of water	11.1	10.6
7	Soundness	sound	sound
8	Time of Set, hours	less than 10	less than 10
9	Yield; Dry Material per Cubic Foot of Mortar, lb	127	125
10	Cost of Dry Material per Cubic Foot of Mortar, cents	15.8	14.1
	Compressive Strength.		
11	7 Days Old, in Air, lb. per sq. in	1800	1400
12	6 Months " " " " "	2600	2000
13	6 " " " Water, " "	4100	3000
	Tensile Strength.		
14	6 Months Old, in Air, lb. per sq. in	275	220
15	6 " " " Water, " "	360	295

Precautions.

These experiments were made with a Portland cement which passed the United States Government specifications, a high-magnesian hydrate which passed the A.S.T.M. Standard Specifications for Hydrated Lime, and standard Ottawa sand. Enough water was added to give normal consistency by the ball method. The use of a high-calcium hydrate should not materially affect the results. A well-graded building sand will probably give higher strengths.[1]

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[1] For further information on these points, see Emley, "Effect of Consistency and Amount of Sand on the Properties of Lime Mortars," *Transactions*, Am. Ceramic Soc., Vol. 16 (1914); Emley and Young, "The Strength of Lime Mortar," *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part II, p. 338 (1914); "Uses of Hydrated Lime in Portland Cement Mortar," *Transactions*, Nat. Lime Mfgrs. Assn., Vol. 11 (1913).

DISCUSSION.

Mr. Scofield.

Mr. H. H. Scofield (*by letter*).—The writer has been engaged for some time in the study of the properties of cement-lime-sand mortars, particularly with reference to their use in concrete. There seems to be, therefore, in the opinion of the writer an objection to the use of standard Ottawa sand mortar to give information concerning the properties of commercial mortars containing hydrated lime. The properties of a mortar are probably as much dependent upon the grading of the fine aggregate used as on any other single item. In this respect Ottawa sand is very dissimilar to either commercial sand or commercial screenings. Ottawa sand has no particles approaching the size of particle of hydrated lime or cement, while commercial sands and screenings are likely to have a sufficiency, if not an excess, of particles approaching that size. The effect of the addition of hydrated lime on the density and strength of commercial mortars might therefore be entirely opposite to its effect on Ottawa sand mortar of the same proportion.

As a matter of information, the results of one of the series in process of investigation at Purdue University might be cited. The series involves the determination of density and strength of a rich, medium and lean mortar with bank sand and limestone screenings of three different gradings, that is, straight line or "uniformly graded," medium fines present, excess fines present. The hydrated lime additions are 0, 10, 20 and 30 per cent by weight of cement added.

For purposes of brevity, only the results for the average grading, fine aggregate containing a medium amount of fines, are here given in comparison with the Ottawa sand results as given by Mr. Emley. The grading selected as average was as follows:

100	per cent passes	$\frac{1}{4}$-in. sieve
72	" "	No. 8 "
50	" "	No. 16 "
35	" "	No. 30 "
5	" "	No. 100 "

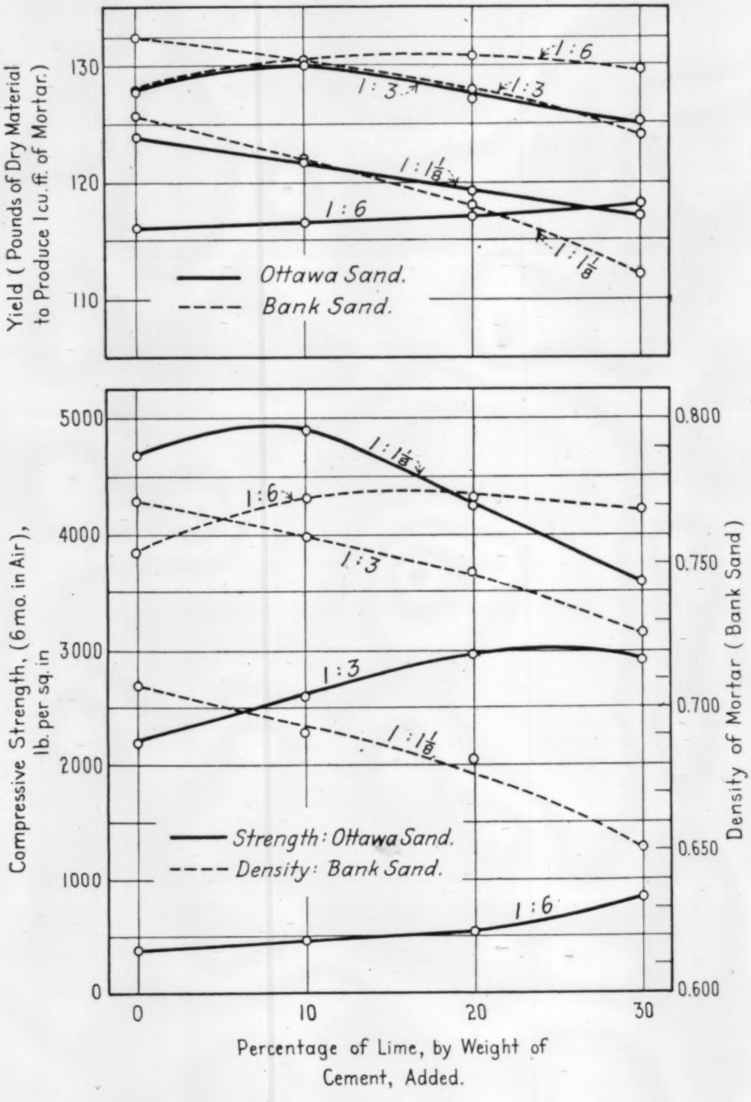

FIG. 1.—A Comparison of Ottawa Sand and Bank Sand in Mortars with Hydrated Lime Additions.

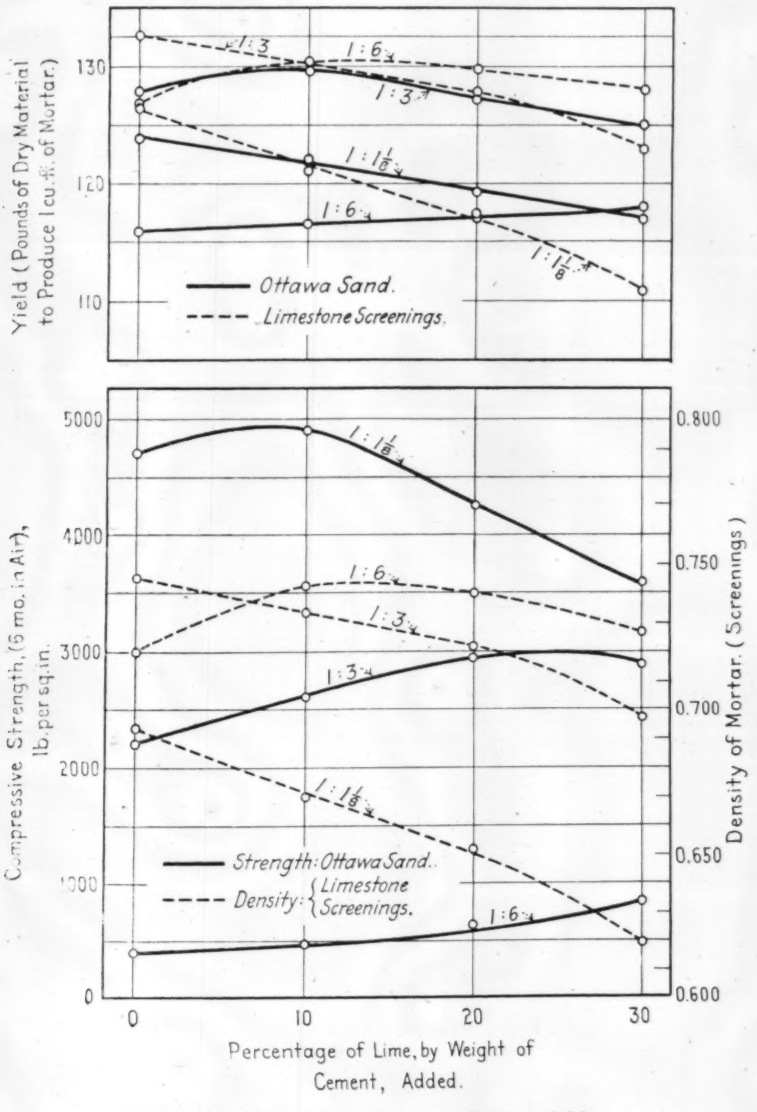

FIG. 2.—A Comparison of Ottawa Sand and Limestone Screenings in Mortars with Hydrated Lime Additions.

Mr. Scofield. All batches of aggregate were identical for a given grading.

Fig. 1 gives the comparison in yield, strength and density of Ottawa sand mortars as interpolated by the writer from Mr. Emley's diagrams, and the same properties as found by the writer using an average bank sand. Fig. 2 is a similar comparison using limestone screenings of the same grading. The lower portion of each figure compares strength of Ottawa sand mortar with the density of the sand and screenings mortar. This is necessitated by the fact that the strength tests of the latter have not yet been made. However, for the same proportion of cement and the same consistency, the strength and density are proportional and the comparison as given is justifiable.

It will be noted that highest yield—or what is essentially the same thing, highest density—occurs in Ottawa sand mortars at 0 per cent lime for the 1 : $1\frac{1}{8}$ mortars, 10 per cent lime for the 1 : 3 mortars, and 30 per cent or more for the 1 : 6 mortars. In this connection, it might be said that a parallel series by the writer using Ottawa sand checks Mr. Emley's results exactly for the 1 : 3 mortars. For the other two proportions, the same relative values were obtained by the writer but the individual values were somewhat higher.

However, in the cases of the bank sand and limestone screenings, no increase in the density and yield is obtained by the addition of hydrated lime in a mortar as rich as 1 : 3 by weight. The 1 : $1\frac{1}{8}$ mortar shows a decided falling off in density and yield with the addition of lime. The lean 1 : 6 mortar exhibits a maximum yield and density at 10 or 15 per cent lime, as might be expected.

As a matter of added information, it might be said that the straight or "uniformly graded" fine aggregates gave the same relative results to a less degree, showing less differences between individual values.

The fine aggregate containing an excess of fines showed the same relative results except that the addition of lime greatly augmented the decreased density and yield. The sand illustrated is considered of average grading, although many sands used in mortars are much finer. The use of a finer sand, however, results in a greater loss in density as lime is added.

MR. WARREN E. EMLEY (*Author's closure by letter*).—Mr. Scofield evidently believes that an addition of hydrated lime would be detrimental to a mortar already containing an excessive amount of fine material, either as cement or as sand. A careful study of the facts which he has presented has failed to disclose the basis for such an opinion. **Mr. Emley.**

At first glance, it seems plausible that a point of maximum density could be obtained with a certain definite proportion of fine material: that either more or less fine material than this definite proportion will produce a less dense mortar. That this is so, is aptly illustrated by Mr. Scofield's curves. It is well to remember, however, that there are many factors other than the sizing of the grains which influence the density of the mortar. Among them may be mentioned the specific gravity of the ingredients, the quantity of water used, the plasticity as affecting the workmanship, the volume change while hardening, etc. That the influence of none of these factors was felt in Mr. Scofield's work can hardly be taken as proof that they have no effect. I believe, therefore, that it is impossible to draw a general conclusion expressing the relation between size of grain and density, without regard to any other consideration.

Mr. Scofield states that the point of maximum yield is coincident with the point of maximum density. This is conceivably true in cement-sand mortars, but not when lime is added to them. For example, it is quite possible to make a cement-sand mortar and a lime-sand mortar having the same yield, but vastly different densities. The addition of lime to a cement-sand mortar cannot act alone, but must always be accompanied by a change in the amount of water required to bring the mortar to a definite consistency, a change in plasticity, and similar factors, all of which exert more or less influence on both the density and the yield of the mortar.

He states that "for the same proportion of cement and the same consistency, the strength and density are proportional," and on this assumption, infers conclusions as to the effect of hydrated lime on the strength of the mortar. As I stated above, I do not believe he has presented sufficient evidence to demonstrate the effect of hydrated lime on the density of the mortar, and therefore any conclusion, drawn by analogy, as to its effect

Mr. Emley. on the strength must be subject to the same criticism. Furthermore, the strength is not always proportional to either the yield or the density, as may be seen by an inspection of Mr. Scofield's own figures.

The object of my paper was twofold: first, to show the effect of hydrated lime in cement mortars under certain definite conditions; and second, to illustrate the value of the triaxial diagram as a means of studying the properties of mortars. The greatest care must be exercised when interpreting my results, to apply them to different conditions. I hope and believe that Mr. Scofield will be able to develop some rules by means of which such an interpretation may be made with assurance.

ECONOMICAL PROPORTIONS FOR PORTLAND-CEMENT MORTARS AND CONCRETES.

By J. A. Kitts.

SUMMARY.

In 1915 a paper by the writer was presented before this Society, entitled "Weight-Volumetric Proportioning of Concrete Aggregates in Testing."[1] The paper described a method of accurate proportioning by volume and outlined the scientific procedure necessary to determine the proportions of the aggregates giving maximum efficiency. The following statements were made in that paper bearing upon the topic of this paper:

"A porous mortar or a porous concrete is not an economical mixture with reference to cost per unit of strength. A mortar with cement greatly in excess of the volume of voids in the sand is not an economical mixture. A concrete with mortar greatly in excess of the volume of voids in the rock is not an economical mixture. The vital changes in density, permeability, and cost per unit of strength take place by variations in the volume of the filling medium when this volume is in the vicinity of that of the voids."

Data are presented in this paper to more exactly define the limits for economical proportions.

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 153 (1915).

ECONOMICAL PROPORTIONS FOR PORTLAND-CEMENT MORTARS AND CONCRETES.

By J. A. Kitts.

The Void Theory.

As an initial step in the determination of the laws of concrete mixtures, it is more important that one combination of materials be thoroughly studied in a great number of progressive proportions with enough test specimens of each mixture to properly reduce the error of the average results, than it is to study a large number of combinations of a large number of materials with only a few mixtures and a few test specimens of each combination.

With a certain combination of cement, sand and rock, we find that an arbitrary 1 : 2 : 4 mixture shows greater strength in proportion to cost than a 1 : 3 : 6 mixture, and that a 1 : 2½ : 6 mixture is more efficient than either. It is probable that no one of these arbitrary mixtures is the most efficient and it is apparent that any number of arbitrary mixtures may be made and tested with the common result of producing more confusion than knowledge of the laws of mixtures. The arbitrary method of proportioning has been absolutely barren of positive results and it will be admitted that some other methods must be resorted to, if we are to arrive at these laws.

The void theory of proportioning has been known for a number of years but has not been given proper consideration, and the fundamental principle has not been appreciated. This lack of consideration has been due to the fact that the theory, as generally known, defined the proportions for maximum strength rather than for maximum efficiency or maximum strength in proportion to cost. A broad statement of the void theory is as follows:

The maximum efficiency proportion for a mortar or a concrete is controlled largely by the proportions of voids in the respective aggregates.

This aspect of the void theory is the basis of the following considerations.

MORTARS.

A mortar is a mixture of sand, cement and water in various proportions. The question of scientific interest is: What determines the mixture of maximum efficiency?

Sands vary in physical, chemical and mechanical structure, causing a variation in the specific gravity and percentage of voids, and these latter cause a variation in the weight per cubic foot or aggregate specific gravity. An important consideration in the study of a large number of mortars from a large number of aggregates is that of comparison. A study of the characteristics of various sands will show that mortars are not comparable either in arbitrary weight proportions or in arbitrary volumetric proportions. What then determines the conditions for comparison of the mortar from one sand with that from another?

An analysis of the results indicated in Prof. M. O. Withey's tests of mortars[1] throws considerable light on the two preceding questions. In these tests mortars were made in 1 : 2, 1 : 3, 1 : 4 and 1 : 5 weight proportions and the following tests made: Unit tensile strength; unit compressive strength; leakage of water through specimens 2 in. thick with pressures of 10 and 40 lb. per sq. in.; density; yield; and compressive strength in proportion to cost. Tables I and II show the physical and mechanical characteristics of eleven of the sands used in these tests.

Tables III and IV show the variations of the volumetric and void conditions common to simple weight proportioning.

Table III is computed by the following equation:

$$\frac{\text{Agg. Volume Sand}}{\text{Agg. Volume Cement}} = \frac{\text{Wt. Proportion Sand}}{\text{Wt. Proportion Cement}} \times \frac{\text{Agg. Sp. Gr. Cement}}{\text{Agg. Sp. Gr. Sand}} \quad . (1)$$

$$\text{Aggregate Specific Gravity}^{2} = (1 - \text{Proportion of Voids}) \times \text{Specific Gravity} \quad . (2)$$

$$= \frac{\text{Weight, in lb. per cu. ft.}}{62.5}$$

$$= 110/62.5 = 1.76 \text{ for cement.}^{3}$$

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[1] M. O. Withey, "Tests of Mortars Made from Wisconsin Aggregates," *Proceedings*, Am. Soc. Test. Mats., Vol. XIII, p. 834 (1913).

[2] The writer called this term "Apparent Specific Gravity," in his paper presented in 1915, previously referred to, for lack of a better term.

[3] 110 lb. of cement makes 1 cu. ft. of normal-consistency paste.

Table I.—Properties of Sands. (From Withey's Table I.)

Sand No.	Weight per Cubic Foot, lb.	Specific Gravity.	Voids, per cent.	Silt, per cent.	Absorption, per cent.
Standard	104.1	2.65	37.0	...	....
Sd. 1	105.2	2.66	36.5	3.0	0.19
Sd. 2	106.9	2.74	35.2	1.3	0.49
Sd. 3	101.8	2.68	38.2	0.8	0.27
Sd. 4	98.9	2.63	39.8	1.2	0.04
Sd. 5	91.2	2.67	45.3	0.5	0.17
Sd. 7	105.2	2.78	36.6	1.6	0.41
Sd. 8	105.3	2.70	36.4	1.5	0.18
Sd. 9	105.5	2.75	36.0	0.7	0.51
Sd. 10	120.3	2.77	27.9	7.7	0.29
Sd. 11	108.7	2.72	35.0	0.4	0.11

Table II.—Sieve Analysis of Sands. (From Withey's Table II.)

Sand No.	Percentage by Weight Passing Sieves Nos.							Uniformity Coefficient.
	10	20	30	40	50	74	100	
Standard	100.0	100.0	0.0	....	....	...	...	...
Sd. 1	86.4	65.5	39.0	25.0	9.1	5.4	2.8	3.0
Sd. 2	81.2	70.9	61.4	52.4	34.5	17.9	9.7	2.6
Sd. 3	91.9	72.7	39.7	26.4	13.7	4.2	1.2	2.4
Sd. 4	100.0	99.5	95.8	88.8	62.5	22.1	8.6	1.8
Sd. 5	100.0	99.9	99.8	99.1	67.6	18.2	5.7	1.8
Sd. 7	67.7	44.5	25.9	17.8	11.2	3.8	2.2	4.7
Sd. 8	82.0	72.5	55.3	38.6	16.1	3.6	1.9	2.6
Sd. 9	66.8	22.7	13.9	10.3	4.9	2.4	1.4	3.7
Sd. 10	69.7	48.8	34.0	26.3	17.4	8.7	6.5	6.9
Sd. 11	72.0	47.7	23.8	12.0	4.3	0.7	0.5	3.4

TABLE III.—VOLUMETRIC PROPORTIONS FOR SIMPLE WEIGHT PROPORTIONS.

Sand No.	Weight Proportions.			
	1 : 2	1 : 3	1 : 4	1 : 5
Sd. 10	1 : 1.77	1 : 2.65	1 : 3.54	1 : 4.42
Sd. 2	1 : 1.98	1 : 2.97	1 : 3.96	1 : 4.94
Sd. 11	1 : 1.99	1 : 2.98	1 : 3.97	1 : 4.97
Sd. 7	1 : 2	1 : 3	1 : 4	1 : 5
Sd. 9	1 : 2	1 : 3	1 : 4	1 : 5
Sd. 8	1 : 2.05	1 : 3.07	1 : 4.10	1 : 5.12
Sd. 1	1 : 2.08	1 : 3.12	1 : 4.17	1 : 5.20
Standard	1 : 2.11	1 : 3.17	1 : 4.22	1 : 5.27
Sd. 3	1 : 2.13	1 : 3.20	1 : 4.27	1 : 5.33
Sd. 4	1 : 2.23	1 : 3.34	1 : 4.46	1 : 5.57
Sd. 5	1 : 2.41	1 : 3.62	1 : 4.82	1 : 6.03

TABLE IV.—RATIO OF VOLUME OF CEMENT PASTE TO VOLUME OF VOIDS IN SAND FOR SIMPLE WEIGHT PROPORTIONS.

Sand No.	Weight Proportions.			
	1 : 2	1 : 3	1 : 4	1 : 5
Sd. 10	2.03	1.35	1.01	0.81
Sd. 11	1.44	0.96	0.72	0.58
Sd. 2	1.43	0.96	0.72	0.57
Sd. 9	1.39	0.93	0.70	0.56
Sd. 7	1.37	0.91	0.68	0.55
Sd. 8	1.34	0.90	0.67	0.54
Sd. 1	1.32	0.87	0.66	0.53
Standard	1.28	0.85	0.64	0.51
Sd. 3	1.23	0.82	0.61	0.49
Sd. 4	1.13	0.75	0.56	0.45
Sd. 5	0.91	0.61	0.46	0.37

Table IV is computed by the following equation:

$$\frac{\text{Vol. of Cement Paste}}{\text{Vol. of Voids in Sand}} = \frac{\text{Agg. Sp. Gr. of Sand}}{\text{Agg. Sp. Gr. of Cement} \times \text{Wt. Proportion of Sand} \times \text{Voids in Sand}} \quad \ldots (3)$$

Table III shows that the volumetric proportions corresponding to the 1 : 2 weight proportions vary from 1 : 1.77 to 1 : 2.41, the 1 : 3 weight from 1 : 2.65 to 1 : 3.62 volume, 1 : 4 weight from 1 : 3.54 to 1 : 4.82 volume, and the 1 : 5 from 1 : 4.42 to 1 : 6.03. Table IV shows the proportions of voids in the sands filled with cement paste varying from 0.91 to 2.03 for the 1 : 2 weight proportion, from 0.61 to 1.35 for the 1 : 3, from 0.46 to 1.01 for the 1 : 4, and from 0.37 to 0.81 for the 1 : 5 proportion. Nothing could better illustrate the fallacy of the practice of comparing work sands with standard sand in 1 : 3 weight proportions.

TABLE V.—RATIO OF VOLUME OF CEMENT PASTE TO VOLUME OF VOIDS IN SAND, FOR SIMPLE VOLUMETRIC PROPORTIONS.

Sand No.	Volumetric Proportions.			
	1 : 2	1 : 3	1 : 4	1 : 5
Sd. 5	1.10	0.73	0.55	0.44
Sd. 4	1.25	0.84	0.63	0.50
Sd. 3	1.31	0.87	0.65	0.51
Standard	1.35	0.90	0.68	0.54
Sd. 7	1.35	0.91	0.68	0.55
Sd. 1	1.36	0.91	0.68	0.55
Sd. 8	1.37	0.92	0.69	0.56
Sd. 9	1.39	0.93	0.69	0.56
Sd. 2	1.42	0.95	0.71	0.57
Sd. 11	1.43	0.95	0.71	0.57
Sd. 10	1.79	1.20	0.90	0.72

$$\frac{\text{Volume of Cement Paste}}{\text{Volume of Voids in Sand}} = \frac{1}{\text{Vol. Prop. of Sand} \times \text{Prop. of Voids in Sand}} \quad \ldots (4)$$

Neither is there a basis of comparison in a fixed volumetric proportion. In 1 : 3 volumetric proportions the proportions of the voids in the sands filled with cement paste would vary from 0.73 to 1.20 for the eleven sands as shown in Table V. It must be admitted, from a scientific or practical consideration of a mortar, that a very important function of the cement is to fill the voids in the sand; and it cannot be expected that a mortar in which the volume of cement paste is equal to only 73 per cent of the volume of voids in the sand, is comparable with that in which the cement paste is equal to 120 per cent of the voids.

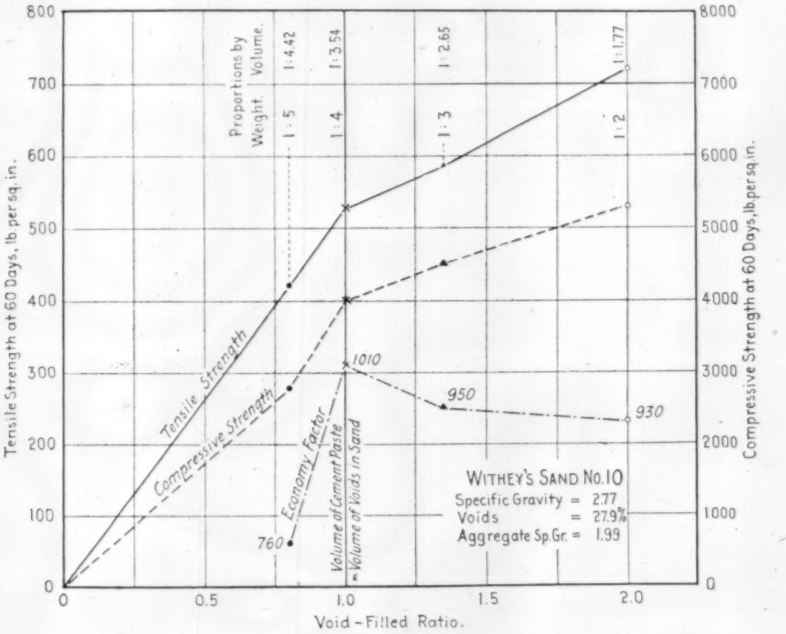

FIG. 1.—Variation in Strength of Mortar from Sand No. 10 in Proportion to Void-Filled Ratios.

Assume, for example, that six mortars of a certain sand and cement are made in which the ratio of the volume of cement paste to the volume of voids in sand has, respectively, the values 0.50, 0.75, 1.00, 1.50, 2.00 and 3.00. It is reasonable to anticipate that the strength of the mortars will increase at a rapid rate until the voids in the sand are filled with cement paste, after which the strength will increase at a lesser rate until the sand particles are so widely separated by the cement paste that the strength of the mortar will closely approximate the strength of the neat cement. In other words, as the cement content is increased, the rate of increase of strength is greater before the voids are filled than it is after the voids are filled, and in some finite proportions the strength of the mortar approximates the strength of the neat cement. Fig. 1, which shows the compressive and tensile strengths and the strength in proportion to cost for sand No. 10, plotted against the void-filled ratio, illustrates these assumptions very well. This sand, unfortunately, is the only one of this series of tests which has a proportion (1 : 4 by weight) closely equal to that in which the volume of cement paste is equal to the volume of voids in the sand, and which has more than one proportion in which the volume of cement paste is greater than the volume of voids in the sand.

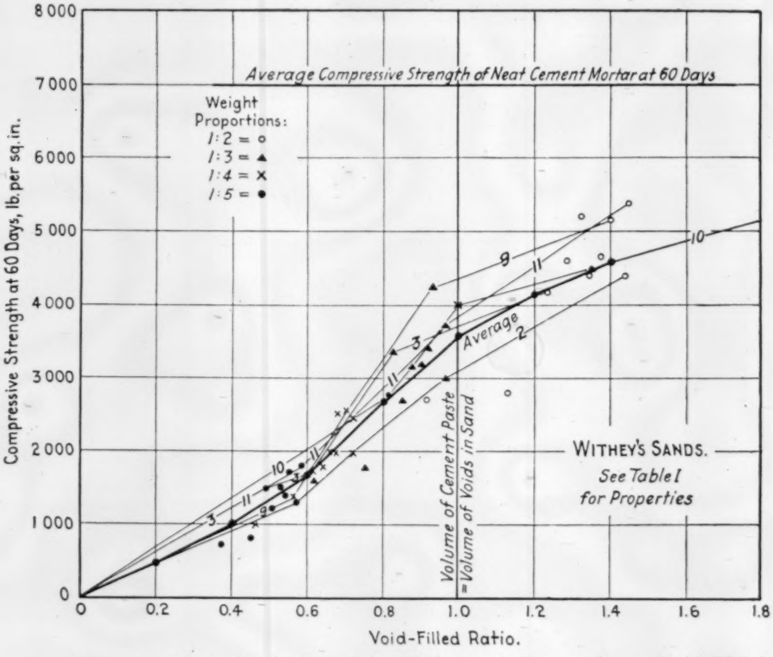

FIG. 2.—Variation in Strength of Mortars in Proportion to Void-Filled Ratios.

Fig. 2 shows the compressive strengths plotted against the void-filled ratio. The curve of averages supports the assumption as to the rate of increase of strength. The curve of averages is fairly uniform, and would appear to indicate that the void-filled ratio has a similar effect on all the sands. This, then, appears to establish the principle that the properties, strength, efficiency, etc., of mortars, are properly compared on the basis of the void-filled ratio.

The final and most important consideration of a mortar is the strength in proportion to the cost. This may be expressed as an "economy factor," equal to the compressive strength in pounds per square inch divided by the cost of the mortar in dollars per cubic yard. This factor may be expressed by the following equation:

$$\text{Economy Factor} = \frac{\text{Compressive Strength (lb. per sq. in.)}}{\text{Cost of Mortar (dollars per cu. yd.)}}$$

$$= \frac{\text{Compressive Strength} \times \text{Yield}}{C_s + \dfrac{P_c \times C_c}{P_s}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (5)$$

in which P_c and P_s are the volumetric proportions of cement and sand, C_c and C_s are the costs, in dollars per cubic yard, of cement and sand, and the yield is based on the volume of the sand as unity.

This factor is plotted in Fig. 10 of Withey's paper, previously referred to. The cost represents costs of materials only, cement being estimated at \$1.50 per barrel and sand at \$1.25 per cubic yard. In Fig. 3 these factors for all eleven sands have been plotted against the void-filled ratio. The average compressive strength of the cement used in these tests was less than 7000 lb. per sq. in. at 60 days. The economy factor for the neat cement mortar would then be approximately $7000 \div 10.14 = 690$[1]. The highest economy factor shown on the curve of averages is 860. This and the curve of averages indicate that the economy factor decreases when the void-filled ratio is somewhat in excess of 1.5.

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[1] It should be noted here that this value for the "economy factor" of neat cement mortar, as well as the other factors in Fig. 3, is based upon the assumption that 1 bag of cement gives 1 cu. ft. of cement paste. If, as has been done in this paper, it is assumed that it requires 110 lb. of cement to make 1 cu. ft. of neat cement paste, the cost of a cubic yard of cement mortar would be \$11.85 instead of \$10.14, giving an "economy factor" of 590.

The results of these tests, therefore, indicate that the most economical mixtures lie between proportions giving a void-filled ratio from 1 to 1.5.

As an illustration, Fig. 3 shows that the most economical proportion for sand No. 10 is that in which the volume of cement paste is equal to the volume of voids. From Fig. 1, it is seen that this proportion is 1 cement : 3.58 sand by volume, giving a tensile strength at 60 days of 525 lb. per sq. in., a compressive strength of 4000 lb. per sq. in., and an economy factor slightly over 1000.

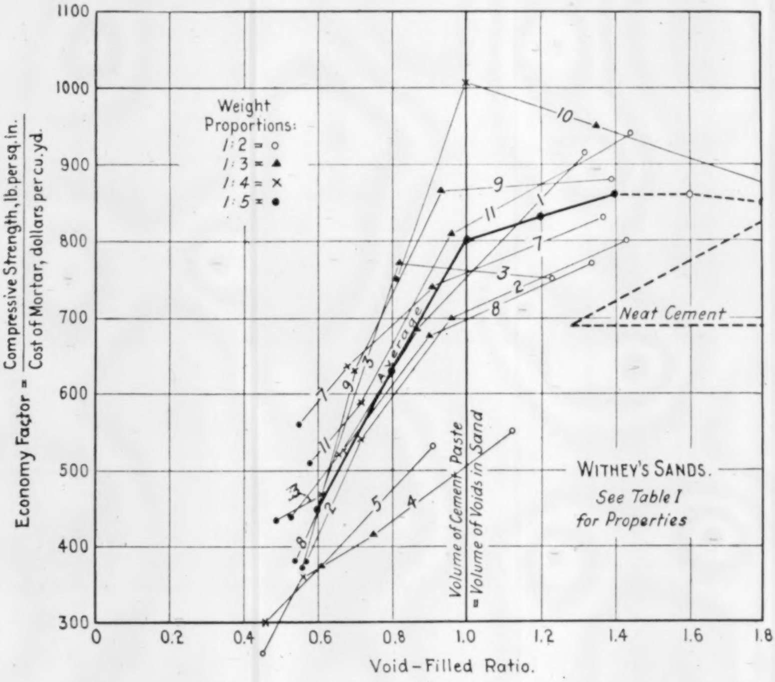

FIG. 3.—Variation in Economy Factor in Proportion to Void-Filled Ratios.

The equations for economical mixtures, as indicated by this series of tests, may therefore be written:

$$\frac{\text{Volume of Sand}}{\text{Volume of Cement}} = \frac{1}{(1 \text{ to } 1.5) \times \text{Proportion of Voids in Sand}} \quad \ldots\ldots\ldots\ldots (6)$$

or

$$\frac{\text{Weight of Sand}}{\text{Weight of Cement}} = \frac{\text{Agg. Sp. Gr. of Sand}}{(1 \text{ to } 1.5) \times \text{Agg. Sp. Gr. of Cement} \times \text{Voids in Sand}} \quad ..(7)$$

If the properties of mortars from all sands vary with the variation of the void-filled ratio, the leakage, density and yield should show similar effects for all sands. Fig. 4 shows the leakage of the various mortars plotted against the void-filled ratio; and this is another proof that the void-filled ratio is the proper basis of comparison of the properties of mortars.

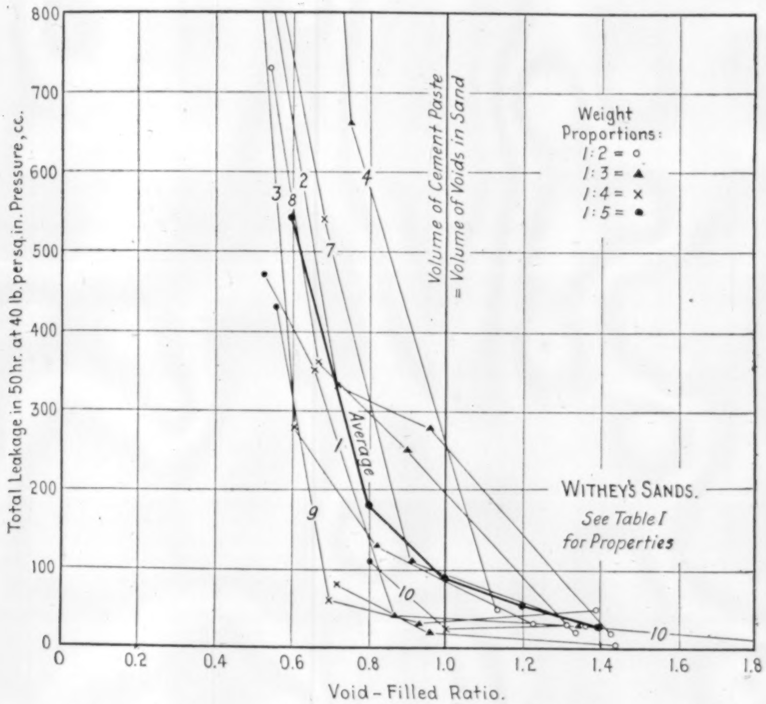

FIG. 4.—Variation in Leakage of Mortars in Proportion to Void-filled Ratios.

CONCLUSIONS REGARDING MORTARS.

1. Sand-cement mortars are not comparable in simple weight proportions because of the wide variations in the corre-

sponding volumetric proportions and the variations of the void-filled ratios.

2. Sand-cement mortars are not comparable in simple volumetric proportions because of the wide variations of the void-filled ratios.

3. The void-filled ratio has a general effect upon the strength, permeability and economy of a mortar and undoubtedly affects the density and yield.

4. An important function of the cement paste is to fill the voids in the sand.

5. Sand-cement mortars are properly comparable on the basis of the void-filled ratios.

6. The economical proportions for sand-cement mortars depend upon the void contents of the sands and may be expressed by Eqs. 6 and 7.

7. The economy factor expresses the relative efficiency of mortars and may be determined by Eq. 5.

8. The writer finds no general relation of silt content, uniformity coefficient, and absorption to the efficiency of sands.

CONCRETE.

In the case where concrete is made from the bank run of gravel, or a concrete mix, and cement, it would be reasonable to expect the same general principles to apply as previously stated for mortars. Where mortar is added to a coarse aggregate, such as rock, it is reasonable to assume that an important function of the mortar is to fill the voids in the rock.

A scientific test to determine the laws of concrete mixtures would be first, to determine the mortar of maximum density and the yield of mortar; second, to make progressive proportions of the mortar with the rock, using values of the ratio of the volume of mortar to the volume of voids in rock equal to 0.50, 0.75, 0.90, 1.00, 1.10, 1.25, 1.50, 2.00 and 3.00 with a view of determining that proportion giving maximum efficiency; and finally, to make additional progressive proportions, maintaining the ratio of fine to coarse aggregate of the "efficient" proportion just determined, but increasing the amount of cement to 1.1, 1.2, 1.3, 1.4, 1.5, 2.0 and 3.0 times that used in the mortar of maximum density. The most economical proportions would be determined by the following equation:

$$\text{Economy Factor} = \frac{\text{Compressive Strength (lb. per sq. in.)}}{\text{Cost of Concrete (dollars per cu. yd.)}}$$

$$= \frac{\text{Compressive Strength} \times \text{Yield}}{C_r + \frac{P_s \times C_s}{P_r} + \frac{P_c \times C_c}{P_r}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (8)$$

P_c, P_s, P_r being the volumetric proportions and C_c, C_s and C the costs in dollars per cubic yard of cement, sand and rock, respectively. The yield is based on the volume of the coarse aggregate (rock) as unity.

There are no test data extant on the economical proportions of concrete. Some tests have been made by the United States Bureau of Standards using the void theory of proportioning in a limited way. The results of these tests will be analyzed in the following discussion and may throw some light on the subject.

The economy of any mixture depends largely upon the local cost of materials. If the cost of cement is twelve to fifteen times that of the aggregates, the economy of leaner mixtures will be more apparent. In Table VI, the economy factor for several 1 : 2 : 4 mixtures is compared with that of several void-theory mixtures, the factors having been computed by the writer from the results shown in Tables 8 and 12 of the Bureau of Standards paper.[1] The void-theory mixtures may be expressed by Eqs. 9 and 10, the void-filled ratio (1 to 1.5) being 1.1 in every case both for the mortar and for the rock. The cost of the aggregates was assumed at \$1.20 per cubic yard and the cost of cement at \$12.00 per cubic yard. The yield of the various mixtures is necessarily assumed as unity, as it is not possible to closely approximate the yield by calculation.

It would appear from a study of this table, that the void-theory mixtures compare quite favorably with 1 : 2 : 4 mixtures, considering efficiency. The two combinations of granite show the 1 : 2 : 4 mixtures as the most efficient. However, there are too few combinations to be conclusive.

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[1] *Technologic Paper No. 58*, U. S. Bureau of Standards.

In Table VII the economy factor is shown for various combinations of aggregates in three mixtures, 1 : 2 : 4, 1 : 3 : 6 and a void-theory mixture. In 33 comparisons, the ratios for the most economical mixtures are as follows:

Theoretical	1 : 2 : 4	1 : 3 : 6
18	15	
30		3
	30	3

TABLE VI.—ECONOMY FACTORS COMPARING 1 : 2 : 4 MIXTURES WITH VOID-THEORY MIXTURES.

Kind of Aggregate.		Age at Test, weeks.					
		4	13	26	52	78	104
		1 : 2 : 4 MIXTURES—2 COMBINATIONS.[1], [4]					
Granite	Minimum	490	630	700	990	....	....
	Average	510	720	710	1010	....	....
	Maximum	540	820	720	1040	....	....
		VOID-THEORY MIXTURES—2 COMBINATIONS.[2], [4]					
	Minimum	410	510	590	880	....	....
	Average	440	620	670	900	....	....
	Maximum	470	730	750	920	....	....
		1 : 2 : 4 MIXTURES—20 COMBINATIONS.[1]					
Limestone	Minimum	260	360	450	780	1140	1280
	Average	570	680	750	990	1260	1410
	Maximum	820	820	940	1360	1520	1500
		VOID-THEORY MIXTURES—20 COMBINATIONS.[2]					
	Minimum	300	390	410	470	1210	....
	Average	600	760	800	890	1330	1710
	Maximum	890	1020	1050	1440	1590	....
		1 : 2 : 4 MIXTURES—10 COMBINATIONS.[1]					
Gravel	Minimum	200	300	390	440	....	....
	Average	550	600	740	810	....	....
	Maximum	870	1040	1090	1190	....	....
		VOID-THEORY MIXTURES—8 COMBINATIONS.[2], [3]					
	Minimum	280	420	460	530	....	....
	Average	560	680	740	820	....	....
	Maximum	1080	930	1110	1220	....	....

[1] From Table 8 of *Technologic Paper No. 58.*
[2] From Table 12 of *Technologic Paper No. 58.*
[3] The two last combinations of Table 12 have been omitted, since they were not common to both mixtures.
[4] Two combinations in common to both mixtures used.

The ratio of cement to total aggregate is 1 : 8.12 for the theoretical mixture, compared to 1 : 6 and 1 : 9 for the 1 : 2 : 4 and 1 : 3 : 6 mixtures. The fact that the 1 : 9 arbitrary mixture is much less efficient than an average 1 : 8.12 theoretical mixture and that the 1 : 8.12 theoretical mixture is somewhat more efficient than the 1 : 6 arbitrary mixture would appear to indicate that there is some basis for the void theory.

The 1 : 2 : 4 mixture generally appears to be a very efficient one and the explanation may be the efficiency of the mortar. As the voids in sands are seldom below 33 per cent or above 50 per cent, the ratio of cement paste to voids for the 1 : 2 mortar varies from 1 to 1.5, which corresponds to the limits previously determined for efficient mortars. Rock seldom shows less than 35 per cent or more than 50 per cent of voids, so that in a 1 : 2 : 4 mixture the volume of mortar will vary

TABLE VII.—COMPARISON OF THE ECONOMY FACTORS OF CONCRETES PROPORTIONED BY THE VOID METHOD AND ARBITRARILY PROPORTIONED MIXTURES 1 : 2 : 4 and 1 : 3 : 6, THE COMBINATION OF AGGREGATES BEING THE SAME IN EACH GROUP.[1]

Kind of Aggregate.	Volumetric Proportions.	Age at Test, weeks.			
		4	13	26	52
Limestone	1 : 2.4 : 4.47	570	720	900	930
	1 : 3 : 6	620	690	750	...
	1 : 2 : 4	700	750	960	...
	1 : 2.4 : 4.45	470	570	680	860
	1 : 3 : 6	410	520	600	800
	1 : 2 : 4	450	590	660	880
	1 : 2.4 : 5.46	800	1000	1050	900
	1 : 3 : 6	790	810	1010	1030
	1 : 2 : 4	600	820	...	1040
	1 : 2.6 : 6.8	640	760	840	1000
	1 : 3 : 6	570	750	820	860
	1 : 2 : 4	830	...	830	1240
	1 : 2.4 : 5.81	670	900	930	710
	1 : 3 : 6	490	730	840	970
	1 : 2 : 4	540	820	810	970
Granite	1 : 2.4 : 4.92	470	720	750	920
	1 : 3 : 6	380	440	520	580
	1 : 2 : 4	540	820	720	980
Gravel	1 : 2.4 : 7.0	640	820	790	...
	1 : 3 : 6	410	540	610	...
	1 : 2 : 4	700	...	920	...
	1 : 2.4 : 6.2	940	930	1110	1220
	1 : 3 : 6	440	590	680	860
	1 : 2 : 4	860	1030	1090	1190
	1 : 2.3 : 6.4	750	930	990	...
	1 : 3 : 6	410	550	650	...
	1 : 2 : 4	700	...	970	...
Average	Void Theory	661	817	893	934
	1 : 3 : 6	502	624	719	850
	1 : 2 : 4	658	805	875	1050

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[1] See Table 13, *Technologic Paper No. 58.*

from 1 to 1.5 times the volume of voids in the rock. A 1 : 3 : 6 mixture is seldom an efficient one because a 1 : 3 mortar is seldom efficient, few sands showing less than 33 per cent voids.

CONCLUSIONS REGARDING CONCRETES.

1. An important function of a mortar in concrete is to fill the voids in the coarse aggregate.

2. The efficiency of a concrete mixture depends largely upon the efficiency of the mortar.

3. For economical proportions the volume of cement should be equal to or greater than the volume of voids in the sand but not to exceed a multiple of 1.5, and the volume of mortar should be equal to or greater than the volume of voids in the coarse aggregate but not to exceed a multiple of 1.5.

4. The equations for economical mixtures may be stated as follows:

$$\frac{\text{Volume of Sand}}{\text{Unit Volume of Cement}} = \frac{1}{(1 \text{ to } 1.5) \times \text{Proportion of Voids in Sand}} \quad \ldots\ldots (9)$$

$$\frac{\text{Volume of Rock}}{\text{Unit Volume of Cement}} = \frac{\dfrac{\text{Volume of Sand} \times \text{Yield of Mortar}}{\text{Unit Volume of Cement}}}{(1 \text{ to } 1.5) \times \text{Proportion of Voids in Rock}} \quad \ldots\ldots (10)$$

5. The economy factor expresses the relative efficiencies of concrete mixtures and may be determined by Eq. 8.

DISCUSSION.

Mr. Abrams. MR. D. A. ABRAMS.—Without going into a detailed discussion of this paper, it seems well to call attention to the author's treatment of the voids in aggregates. Mr. Kitts' entire discussion is based on the theory that the percentage of voids in the aggregate is the criterion by which the properties of the concrete or mortar should be judged. Experimental work carried out in the Structural Materials Research Laboratory, Lewis Institute, Chicago, has shown very clearly that the percentage of voids in the aggregate is by no means a satisfactory criterion for the judgment of the relative strength of concrete. In fact, a most thorough study of this subject has shown that there is no necessary relation between voids and the strength of a concrete made from a given aggregate mixed with a given amount of cement. To carry this criticism a little further, I may say that we have been able to demonstrate from these that, in general, the relation between voids and strength is exactly the reverse of what is ordinarily supposed. This is not an accidental result from incomplete test data, but brings out a systematic relation which has heretofore escaped notice. On the other hand, a direct relation can be secured in certain instances; so that it seems to me that there is a fundamental error in the deductions of this paper, due to the fact that the author has assumed that percentage of voids is a criterion, whereas we are quite sure it is not. That applies to mortar mixtures as well as to concrete mixtures. It would seem to be quite evident that it is impossible to take any arbitrary point in the analysis of an aggregate and conclude that the size below that point fills the voids in the coarser sizes. It seems to me that the absurdity of that conclusion should be clear, since if we attempted to apply this method to sizes finer or coarser, it would become obvious that no such relation could exist.

Mr. Scofield. MR. H. H. SCOFIELD (*by letter*).—The density and strength of concrete are undoubtedly the two most important character-

Mr. Scofield. istics of that material. For practical considerations, the cost per unit strength as stated by Mr. Kitts is a very important item. It follows also that in devising a method of proportioning the materials entering into concrete, one must not lose sight of actual practical conditions of installation.

If it were possible to arrange the sand particles as desired and inject into them the cement paste, to similarly treat the coarse aggregate and inject into it the mortar, and secure the necessary bond of aggregate to mortar, then Mr. Kitts' method would probably be found efficient in every way. However, the quality of mortar and concrete depends upon its characteristics as installed under actual conditions and not upon some imaginary characteristics, desired and considered at the time of measuring the materials. Voids in materials as measured do not represent the voids in those materials after placing in concrete.

TABLE I.

Sand.[1]	Voids, per cent.	Density of 1 : 3 Mortar.	Mortar Strength at One Year, lb. per sq. in.	
			Tension.	Compression.
16 A	36.2	0.724	336	3324
16 B	31.2	0.676	335	3154
16 C	35.8	0.647	328	2556
8 A	34.5	0.788	437	4845
8 B	29.2	0.742	406	3210
8 C	36.9	0.695	356	2768
4 A	30.3	0.828	576	5575
4 B	25.8	0.768	487	5150
4 C	29.2	0.706	386	4174
2 A	28.9	0.855	600	6402
2 B	22.8	0.798	560	5750
2 C	27.8	0.730	416	4409

[1] The numeral before each letter in this column is the number of the sieve through which all of each sand passes.

It is well known that the quality of concrete as installed depends upon many items. Many of these are of more impor-

Mr. Scofield.

tance in their effects than are small differences in voids of aggregates and richness of mortar. The character and grading of the aggregate, the amount of water and duration of mixing, and the method of placing the concrete, are all items of great importance. I believe, therefore, that the voids to be considered should be in all cases the voids in the finished concrete or mortar and not the voids in the materials before mixing. To illustrate, it is logical to believe that a fine aggregate of a certain grading might be selected to fit a certain coarse aggregate and a pro-

TABLE II.

Sand.	Voids (Packed), per cent.	Density of 1 : 3 Mortar.	Mortar Strength at One Year, lb. per sq. in.	
			Tension.	Compression.
A (straight line)	30.2	0.782	571	5751
B	21.8	0.752	559	5551
C	24.0	0.703	450	4654
D	25.8	0.682	363	4073

TABLE III.

Screenings.	Contained Dust, per cent.	Voids, per cent.	Density of 1 : 3 Mortar.
A (straight line)	2.3	35.3	0.771
B	5.0	33.6	0.771
C	10.0	31.8	0.766
D	15.0	29.3	0.756
E	20.0	27.2	0.746
F	25.0	23.6	0.731
G	30.0	21.9	0.729
H	35.0	21.9	0.714
I	45.0	24.4	0.693
J	55.0	27.9	0.671
K	65.0	29.2	0.657
L	75.0	31.8	0.636
M	85.0	33.6	0.616
N	100.0	38.0	0.584

Mr. Scofield. portion be so selected to give an efficient concrete, even though separate tests upon both reveal them to be high in voids.

As a matter of added information, I have reviewed several series of investigations made at Purdue University in recent years to show that strength and density of mortars seem to follow each other, but that they do not necessarily follow the voids of the dry aggregate. In all of these series the relation of voids to strength was not the aim; the voids were measured merely as additional data in each case.

Table I gives the results of a series undertaken primarily to observe the relation between the grading of the fine aggregate and the density and strength of 1 : 3 mortars. The "A" sands are straight line or "uniformly" graded, the "B" sands contain a medium amount of fines, and the "C" sands have an excess of fines present. The numeral gives the number of sieve through which all of each sand passes. It will be noted that the "A" sands in all cases give the greatest density and strength but the medium fine "B" sands show the lowest percentage of voids.

TABLE IV.

NATURAL BANK SANDS.

Sand.	Voids, per cent.	Densities of Mortars.		
		1 : 1.2	1 : 3	1 : 6
A	30.5	0.743	0.820	0.819
B	28.4	0.708	0.772	0.755
C	30.3	0.689	0.734	0.729
LIMESTONE SCREENINGS.				
A	41.2	0.705	0.787	0.760
B	37.8	0.693	0.746	0.720
C	39.1	0.663	0.714	0.691

Table II shows results by another operator in a somewhat similar series. "A, B, C and D" contain increasing amounts of fine silt in the order named.

In Table III are given the results of a series the purpose of which is to show the effect of dust in limestone screenings on the density and strength of a 1 : 3 mortar. Although the strength tests have not as yet been made, I believe that they will follow the densities. It will be noted that low dust content gives maximum density, but that lowest voids occur at a considerably higher dust content. **Mr. Scofield.**

The results in Table IV are taken from a series under way to show the effect of hydrated lime on density and strength of mortar. The no-lime results, only, are here shown. As before, "A" sands are straight line graded up to $\frac{1}{4}$ in., the "B" sands contain a medium amount of fines (not fine dust only, in this case) and the "C" sands an excess of fines.

Referring again to the practical consideration of using the densest concrete, the writer believes that such a proportion of cement and grading of sand may be used to give the densest mortar and that it is entirely practical to use such mortar with any coarse aggregate. If the contractor finds that the mixture is too "stony" to be workable, more mortar of maximum density may be used and the mixture made workable. But it is not practicable in many cases to use an amount of coarse aggregate sufficient to give *maximum* density, for the resulting mixture will be too stony to work. A compromise must always be effected.

MR. J. A. KITTS (*Author's closure by letter*).—Mr. Abrams is in error in stating that my theory is "that the percentage of voids in the aggregate is the criterion by which the properties of the concrete or mortar should be judged." I have presented evidence in this paper to show that the percentages of voids in the aggregates are the properties of those aggregates on which the proportions, for mortars or concretes, should be based. **Mr. Kitts.**

If I had a large number of sands and wished to find the most efficient mortar from each and also the relative efficiency of each sand, I would proportion the mortars by Eq. 7 using 0.75, 1.0, 1.5, and 3.0 as the void-filled ratio (the variable in the denominator). All sands having cement paste equal in volume to 0.75 of the volume of voids in the respective sands would be comparable, and so on. The volumetric proportions

Mr. Kitts. would then be determined from the computed weight proportions by the following equation:

$$\frac{\text{Agg. Vol. of Sand}}{\text{Agg. Vol. of Cement}} = \frac{\text{Wt. of Sand} \times \text{Agg. Sp. Gr. of Cement}}{\text{Wt. of Cement} \times \text{Agg. Sp. Gr. of Sand}}$$

Having recorded the yield of mortar and made the strength tests, the efficiencies of the various mortars would be determined by Eq. 5.

If, for example, the 1.0 mortar from one sand proved to be much less efficient than the 1.0 mortars from the others, and the outstanding feature of this sand was that it had a large percentage of fines, the cause of failure of the mortar could be definitely ascribed to the excess of fines. However, if the mortars were all made in 1 : 3 weight proportions, as in Mr. Scofield's data, the specific gravity and percentage of voids of the poor sand might be such that the volumetric proportions would be considerably less than that of any other of the sands, the proportion of cement paste in proportion to the voids would be greater, and there would be no indication that the presence of the fines was doing any damage.

If Mr. Scofield had given the specific gravity of his sands with his data, I do not doubt that it could be shown that the density of his various mortars was generally dependent upon the percentage of voids in the sands.

To repeat, Mr. Abrams is in error in stating that I have "assumed that percentage of voids is a criterion." I have shown by diagrams and tables, which are the results of an analysis of some very thorough tests, that there are definite relations between the variation of the void-filled ratio and the variation of strength, between the variation of the void-filled ratio and the variation of the economy factor, between the variation of the void-filled ratio and the variation of permeability; and, no doubt, there is a definite relation between the variation of the void-filled ratio and the variation of density. In view of these relations I have devised Eqs. 9 and 10 for economical mixtures. With ordinarily good materials these equations should give reliable results.

EFFECTS OF GRADING OF SANDS AND CONSISTENCY OF MIX UPON THE STRENGTH OF PLAIN AND REINFORCED CONCRETE.

BY L. N. EDWARDS.

SUMMARY.

The physical properties of concrete, like those of Nature's conglomerates, depend primarily upon the cementing material and aggregates of which it is composed. The methods and operations used in the making of concrete are vital factors in the production of conditions affecting the density, strength and permanence of the concrete mass. The materials are too frequently a means to an end without consideration of the end itself.

This paper has for its object the following:

To describe the methods used in a series of experimental tests undertaken to secure information relating to (1) the influence of the grading of sand, (2) the effect of the consistency of mix upon the strength and physical characteristics of the concrete produced, and (3) the effect of varying the time of mixing.

To give the results obtained and the phenomena observed from tests made upon (1) cylinders in which 12 sands of predetermined gradings were used as sand aggregates, (2) cylinders and reinforced-concrete beams in the preparation of which five consistencies of mix were used, and (3) cylinders for which the time of mixing was varied from $\frac{1}{4}$ to 2 minutes.

In the preparation of the test specimens the field conditions accompanying high-grade concrete construction operations were duplicated in so far as practicable. The results obtained show:

1. A wide range in the strengths of concrete produced from sands of different gradings;

2. Important variations in the strength of both plain and reinforced concrete, depending upon the consistency of the mix;

3. The inconsistency of common field methods and operations, and of the usually specified requirements for sand, as opposed to the generally accepted and specified requirements in design.

EFFECTS OF GRADING OF SANDS AND CONSISTENCY OF MIX UPON THE STRENGTH OF PLAIN AND REINFORCED CONCRETE.

By L. N. Edwards.

The rapid development and the wide application of plain and reinforced concrete in modern construction is without parallel in the history of materials. This change of status, from a minor material to one of first importance, has not been characterized by the degree of constructive criticism and conservatism which accompanied the adoption of iron and steel in the construction of bridges, buildings, and allied structures.

Laboratory experiments have multiplied to such an extent that the available data so produced are greater than those existing for any other single construction material. Engineers have sifted, culled and graded this information, and from it have evolved our modern practice in plain and reinforced-concrete design. Concrete has thus been studied mainly as a single material rather than as a combination of constituent materials and mechanical operations; each of which, taken individually, exerts a distinct influence upon the character of the combined product.

A definite knowledge of all the factors which tend to produce widely varying results, affecting the strength and permanence of structures composed wholly or in part of concrete, in the opinion of the author, is necessary to a thorough understanding of the full range of its usefulness. This necessarily involves not only the properties, selection and proportioning of the materials, but also the amount of water to be used, the method and thoroughness of mixing, the manner of placing, the temperature of seasoning, and various other factors affecting the practical operations involved in field construction. In former years the quality of the cement was considered as paramount in the production of concrete, but in the past few years the importance of the careful selection of other materials and of the field conditions attending the mixing, placing, etc., has been more generally recognized. However, aside from the limited investigations of a few individuals, very little information is available.

This paper presents the results of three series of tests made by the City of Toronto, Department of Works, in 1916, under the direct supervision of the author.

OBJECT AND SCOPE OF TESTS.

The tests were undertaken with the object of securing information relating to (1) the influence of the grading of sand, (2) the effect of the consistency of mix upon the strength and physical characteristics of the concrete produced, and (3) the effect of varying the time of mixing.

In so far as consistent with available facilities, the proportioning of concrete materials, the mixing of the concrete, and the preparation of test specimens were to be carried on under conditions which duplicated those usually accompanying actual field construction work under good supervision.

For sand tests, all sands were to conform to predetermined gradings. The usual requirements of sharpness, cleanliness, etc., were to be carefully maintained.

The range or scope of the tests as planned and carried out was divided as follows:

1. To secure information covering the chemical and physical properties of all materials used;
2. The preparation of 12 sands to conform to predetermined gradings and the securing of information showing their relative values in concretes and mortars;
3. To ascertain the influence of varying the quantity of water used in mixing concrete for both plain and reinforced-construction;
4. To ascertain the effect of varying the time of mixing; and
5. To secure miscellaneous general information and data pertaining to the foregoing phases of the tests.

The investigations herein described involve tension, compression and bend tests upon 366 concrete cylinders, 6 in. in diameter by 12 in. long; 75 reinforced-concrete beams, 4 by 6 in. in cross-section by 4 ft. long; 82 standard briquettes; and 120 2-in. cubes.

MATERIALS USED IN TESTS.

The properties of the cement, sand and stone aggregates, and of water used in the preparation of test specimens were as follows:

Cement.—The Portland cement was of Canada brand, manufactured by the Canada Cement Co., Limited. At the beginning of the work, a sufficient quantity for all tests was reserved from a single carload lot.

Following are the physical and chemical properties determined in accordance with the standard specifications of the Society:

PHYSICAL PROPERTIES.

Constancy of volume			O.K.
Specific gravity			3.10
Initial set, minutes			150
Final set, minutes			405
Fineness, per cent retained on	No. 100 sieve		2.2
	No. 200 sieve		22.0
Tensile strength, lb. per sq. in.	neat	24 hrs	419
		7 days	656
		28 days	863
	mortar	7 days	284
		28 days	451

CHEMICAL PROPERTIES.

Loss on ignition, per cent	2.68
Insoluble residue, per cent	0.81
Sulfuric anhydride, per cent	2.17
Magnesia, per cent	2.78

Sand.—All sand used in the tests was of limestone origin, secured from a chosen section of the pits of the Maple Sand and Gravel Co., Maple, Ontario. In its original state the sand existed in three clearly defined strata, which to all appearances, varied only in the degree of fineness of the sand grains. The terms "medium fine," "medium" and "coarse" have been applied to the original sands as indicating, in a general way, their character of grading. The actual gradings as determined by tests are shown graphically in Fig. 1.

The following is the chemical composition of a composite sample of the three original sands:

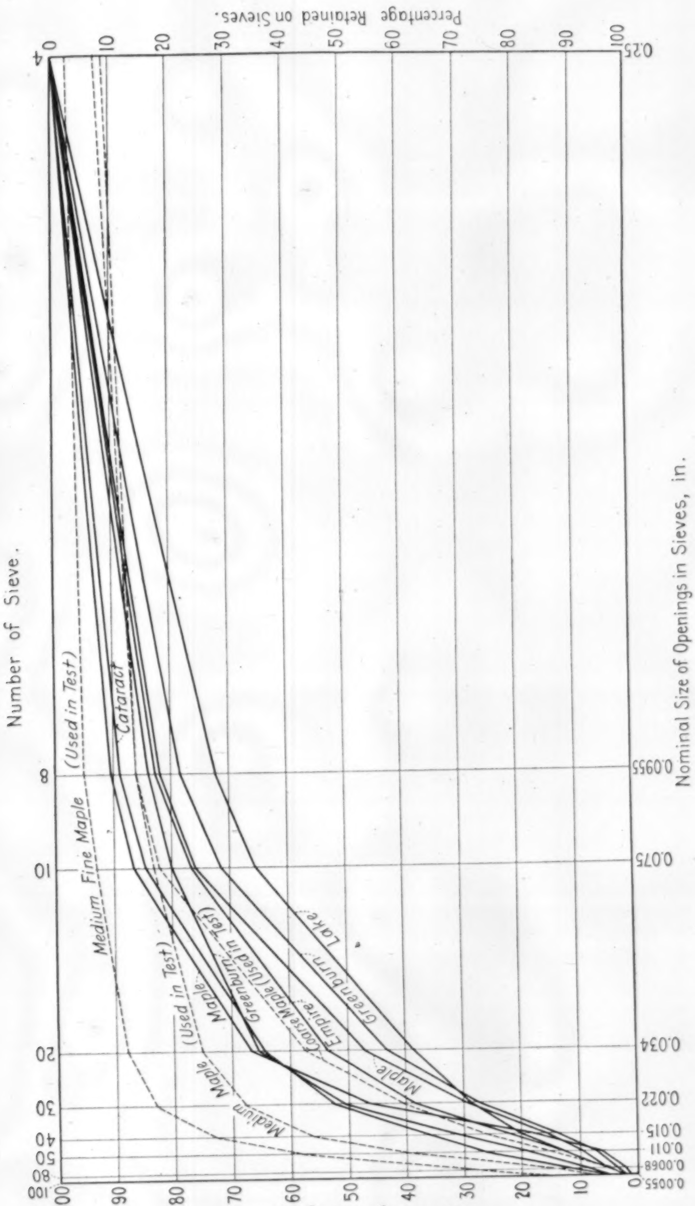


FIG. 1.—Actual Gradings of Natural Sands, Toronto, Ont.

Loss on ignition, per cent	16.32
Silica, per cent	46.97
Iron and alumina oxides, per cent	13.06
Calcium oxide, per cent	15.71
Magnesia, per cent	4.22

The specific gravity of the composite sample taken at 70° F. was 2.684.

The original sands were used only for the purpose of securing therefrom the portions which, when thoroughly mixed together in definite quantities, would produce composite sands agreeing very closely in texture with the predetermined gradings fixed for the test sands.

From each original sand all material passing a sieve having four meshes per linear inch was divided into four parts or portions, as follows:

1. All materials retained upon a No. 10 sieve;
2. All material passing a No. 10 sieve and retained upon a No. 20 sieve;
3. All material passing a No. 20 sieve and retained upon a No. 50 sieve; and
4. All material passing a No. 50 sieve.

The parts so secured were placed in bags and carefully labeled for identification.

Prior to making the division above described the original sands were thoroughly dried upon a metal heater, since the sands as received from the pit were damp. With the object of determining the shrinkage in volume and weight due to drying, 4 cu. ft. of medium sand were measured and weighed, dried, and again weighed and measured. Following are the results of the test:

Average weight of sand, lb. per cu. ft.	wet	92.55
	dry	85.25
Loss in weight (7.30 lb.)		7.9 per cent
Loss in volume (324 cu. in.)		18.7 per cent

Stone.—All crushed stone used in the tests was secured from the Point Anne Quarries Co., Limited, Point Anne, Ontario. The stone supplied by this company is a uniformly dense, dark-colored limestone. Its chemical composition is as follows:

Calcium carbonate, per cent	96.0 – 98.0
Silica, per cent	2.0 – 2.5
Iron and alumina oxides, per cent	0.5
Magnesia	trace
Phosphorus	"

The size of stone used was that commercially known as 1-in. stone. The average grading of this material as received from the storage bins of the company is shown graphically in Fig. 2. The voids in the stone were found to be 43.85 per cent.

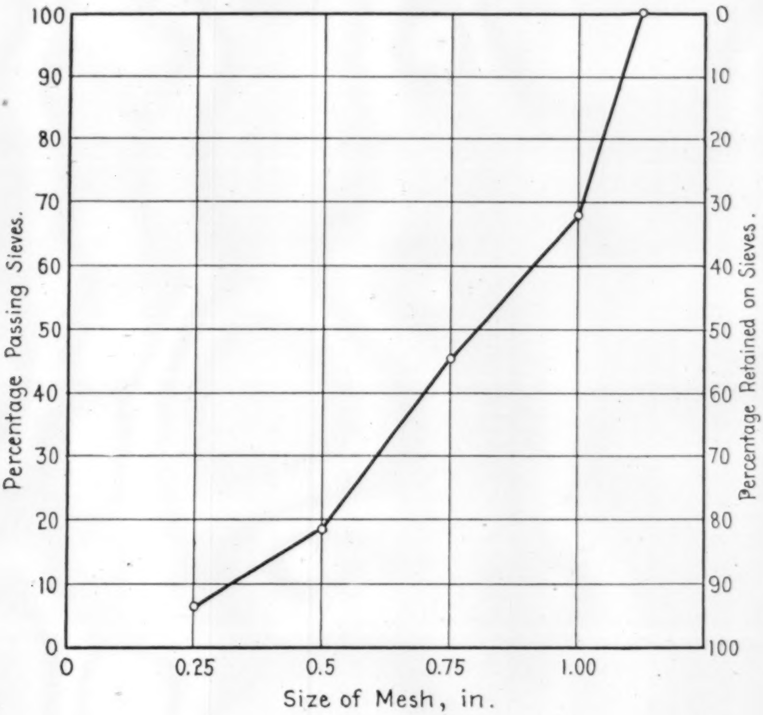


FIG. 2.—Grading of Broken Stone.

The maximum size was approximately 1¼ in. All stone was carefully screened over a ¼-in. screen before using, thus removing all material under ¼ in. in size. With this exception the stone aggregate was used in the concrete mix in the condition in which it was received from the bins. Its uniformity was such that a careful grading seemed unnecessary.

Water.—All water used in the tests was taken from the city water mains, the source of supply being Lake Ontario. The chemical analysis is as follows:

	PARTS PER MILLION.
Total solids	120.0
Alkalinity (lacmoid) bicarbonates 103; carbonates 2	105.0
Permanent hardness	32.5
Total hardness	137.5
Silicious matter	3.84
Iron oxide, alumina and phosphates	0.17
Lime (CaO)	43.4
Magnesia (MgO)	12.2
Sulfates (SO_4)	18.5
Chlorides	9.0

Steel Reinforcement.—The reinforcing steel was used only in the consistency tests upon reinforced-concrete beams.

Following are the physical and chemical properties determined in accordance with the specifications of the Society:

PHYSICAL PROPERTIES.

Tensile strength, lb. per sq. in	72 860
Elastic limit, lb. per sq. in	41 090
Elongation in 2 in., per cent	19.5
Character of fracture	½ cup, silky
Cold bend test	O.K.

CHEMICAL PROPERTIES.

Carbon, per cent	0.26
Manganese, per cent	0.38
Phosphorus, per cent	0.027
Sulfur, per cent	0.033

All steel reinforcement was rolled material. Its surface condition was excellent, showing little evidence of either mill scale or incipient rust.

COMPOSITION AND PREPARATION OF TEST SANDS.

Within a radius of, say, 25 miles from Toronto good limestone sands having a large variety of gradings may be secured with little difficulty. The actual grading of a considerable number of these sands provided a range of grading within which, for practical reasons, it seemed advisable to limit the predetermined gradings of all test sands. Fig. 1 shows graphically the gradings of several natural sands; also, the gradings of the three original sands secured for the tests.

The practical range of sand gradings as above described having been fixed, the compositions for 12 different sands were predetermined as shown in Table I. These gradings are also shown graphically in Fig. 3.

TABLE I.—PREDETERMINED GRADINGS FOR SANDS.

Series No.	Sand No.	Percentages Retained on Sieves.			
		No. 10.	No. 20.	No. 50.	No. 100.
1..........	1	15	35	80	97
	2	20	50	85	97
	3	25	60	95	97
2..........	4	15	40	85	97
	5	20	45	85	97
	6	30	60	85	97
3..........	7	20	40	85	97
	8	25	45	90	97
	9	30	50	95	97
4..........	10	25	55	80	97
	11	25	55	85	97
	12	25	55	95	97

By comparison with Fig. 1, it will be noted that sand No. 2 represents approximately an average of the natural sands, while sands Nos. 1 and 3 represent averages of the finer and coarser grades, respectively.

With the original sands, divided as already described, the quantitites of each required to produce a given sand of the above

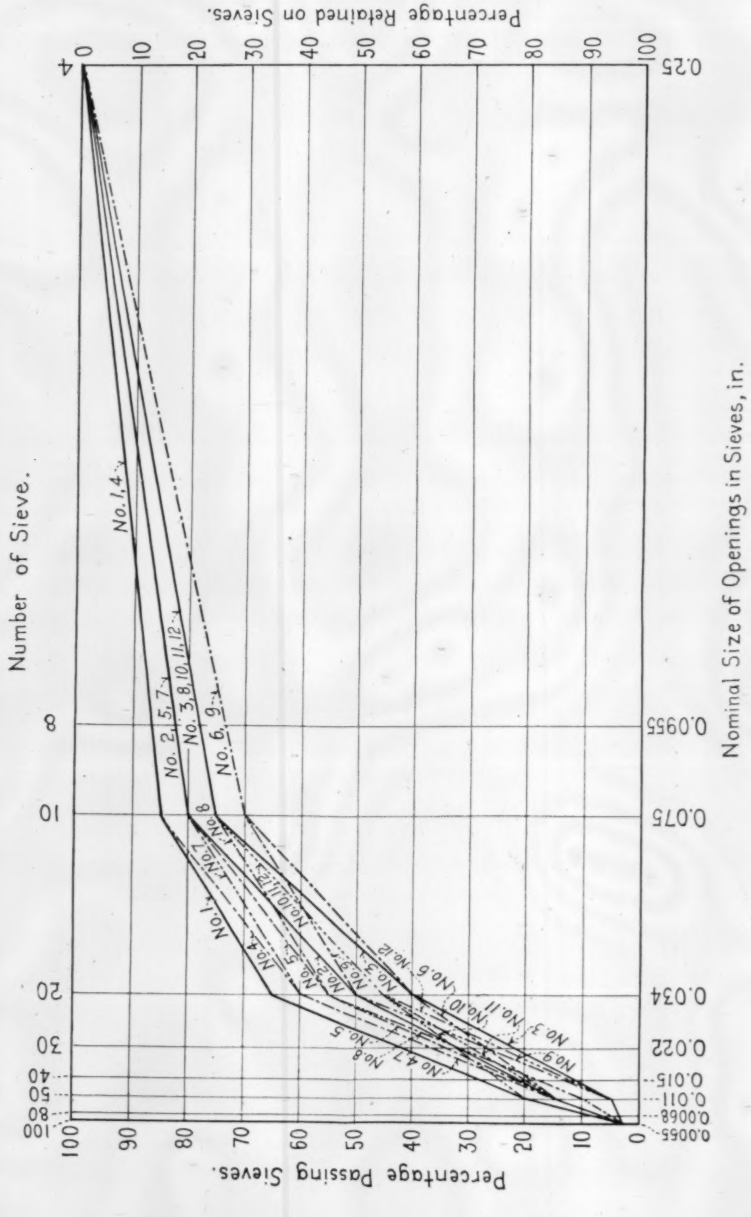


FIG. 3.—Predetermined Gradings of Test Sands.

series was determined by trial. The composition of 100 lb. of sand No. 2 following is typical of the results thus obtained:

Medium Fine	retained upon a No. 10 sieve, lb	26½
Medium Fine	" " " No. 20 " "	23½
Medium	" " " No. 50 " "	17¾
Coarse	" " " No. 50 " "	17¼
Medium	passing " " No. 50 ' "	15
Total		100

The actual grading of the sands used in the tests as determined from "mixer-produced" samples (described later) are shown graphically in Figs. 4 and 5. In this connection, it is of interest to note the relatively small difference existing between the predetermined gradings and the actual gradings.

The art of photography does not readily adapt itself to the reproduction of the impressions obtained by a visual examination of these sands. However, Fig. 6 gives a close approximation of their actual appearance.

The "mixer produced" samples of test sands used for making granulometric analyses, and for the determinations of voids, were each spread out in a thin layer and divided into small squares from which composite samples were taken for use in the tests.

For the granular analysis tests a standard set of 8-in. diameter hand sieves was used, having 8, 10, 20, 30, 40, 50, 80 and 100 openings per linear inch.

The method used for the determination of voids was as follows:

Into a graduated glass cylinder containing 800 cc. of water 1500 cc. of sand were slowly poured, the water being agitated as the addition of the sand proceeded. The sand was allowed to stand in the water for 3 to 5 minutes, when the water level was read. The percentage of voids was then determined as follows:

$$\text{Percentage of voids} = \frac{800 + 1500 - \text{Final volume}}{1500} \times 100.$$

No correction for absorption was made.

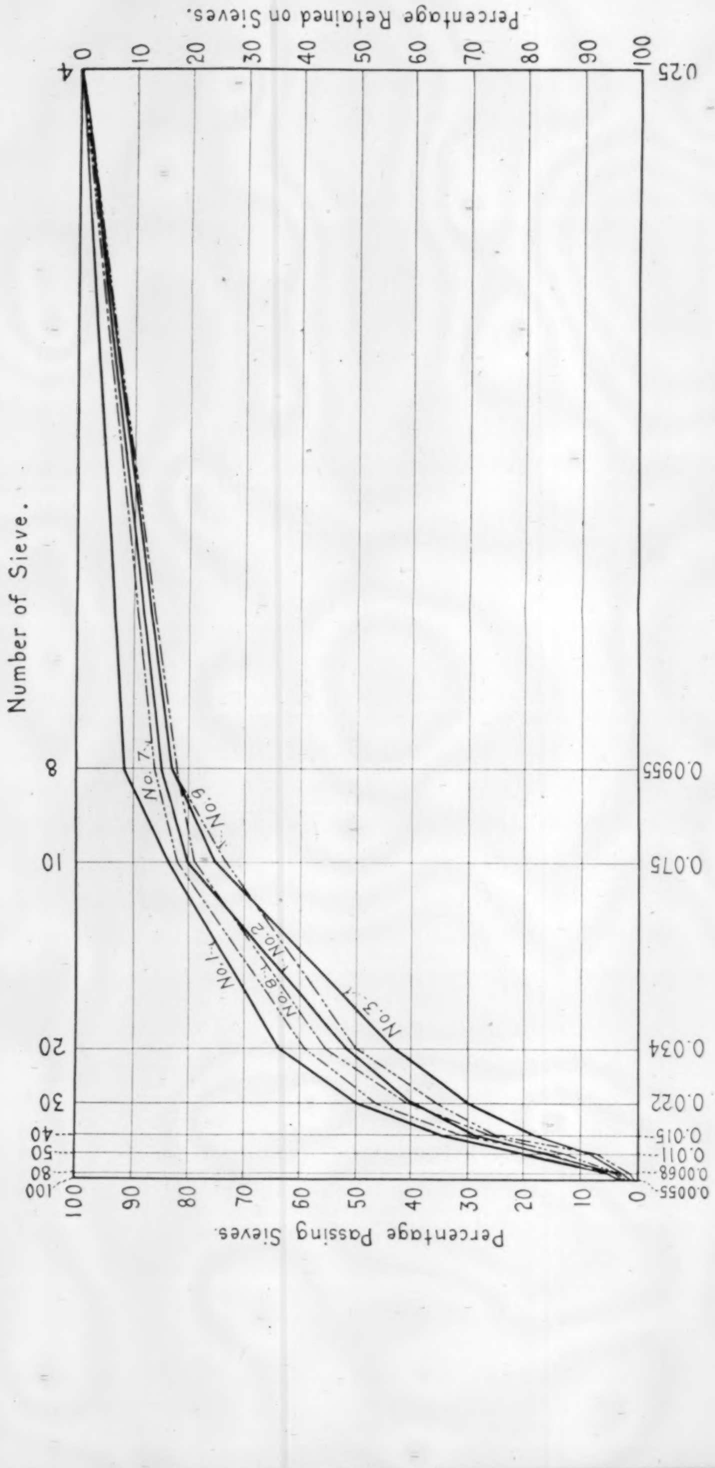


FIG. 4.—Gradings of Test Sands Nos. 1, 2, 3, 7, 8, and 9, as determined from "Mixer-produced" Samples.

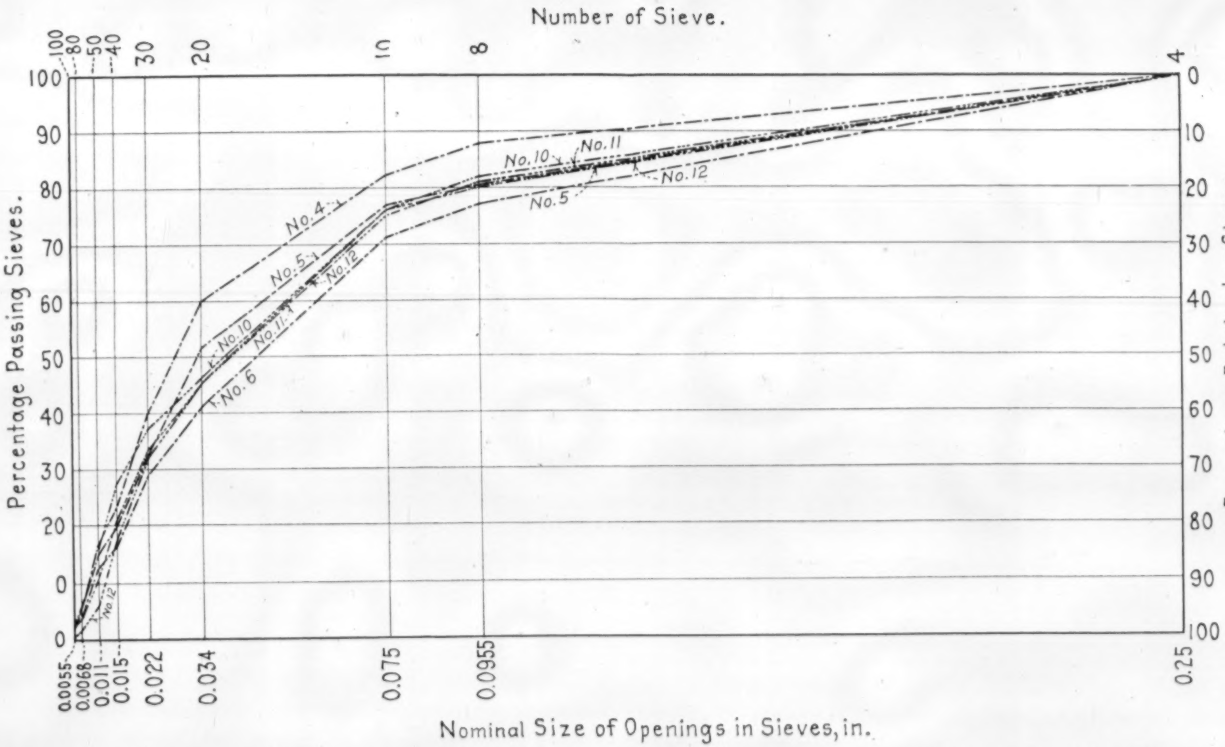


FIG. 5.—Gradings of Test Sands Nos. 4, 5, 6, 10, 11, and 12, as determined from "Mixer-produced" Samples.

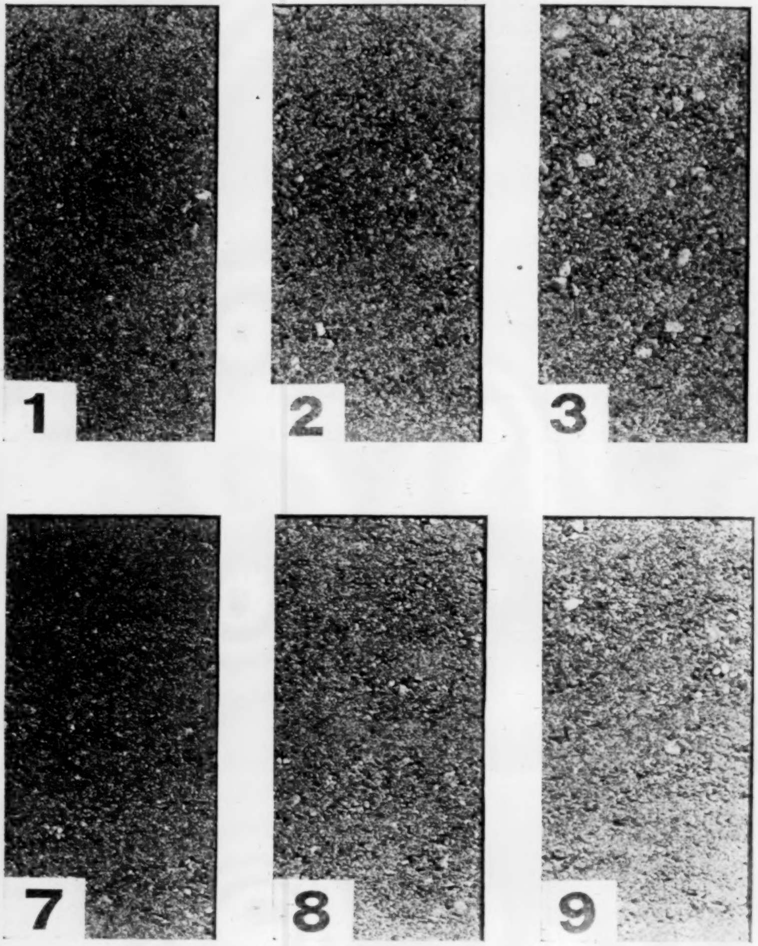


FIG. 6.—Sands used in Tests.

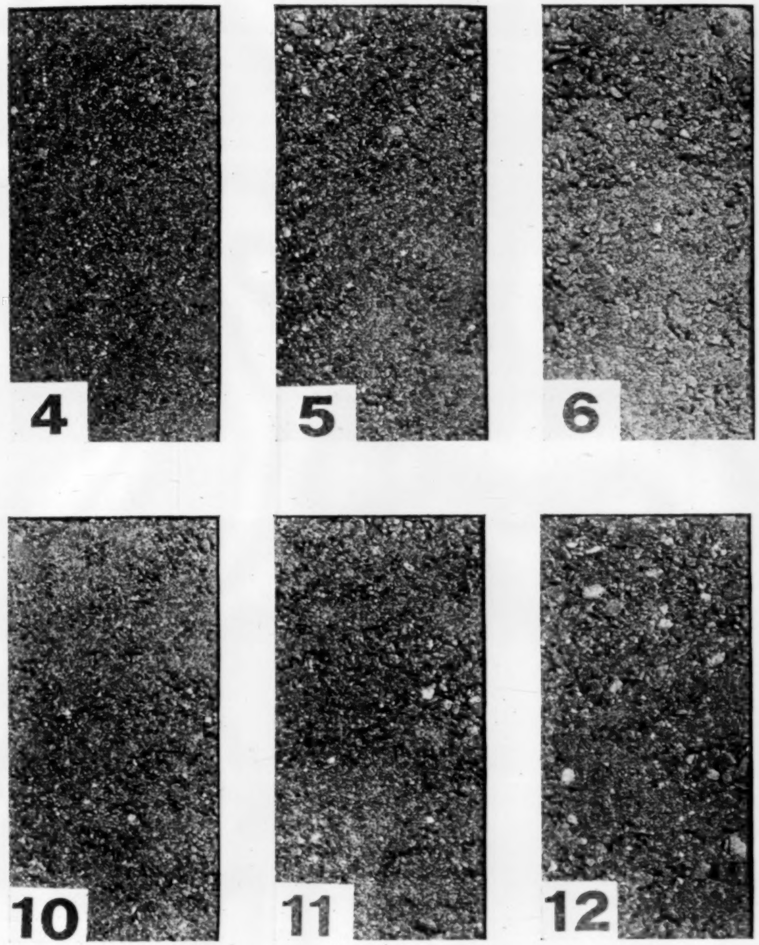


FIG 6.—Sands used in Tests (*Continued*).

CONCRETE PROPORTIONING, MIXING AND PLACING.

The materials entering into the concrete mix were proportioned by volume reduced to a unit-weight basis. The unit weights were as follows:

One cubic foot of cement assumed at	100 lb.
One " " " sand (dry) " "	106.56 "
One " " " stone " " "	89.31 "
One " " " water " "	62.37 "

The unit weights for sand and stone aggregates were determined by taking the average weight of four measures of 1 cu. ft.

As already described, all sand was freed from moisture by heating upon a metal plate. Similarly all broken stone was surface dried whenever found to contain surface dampness.

Proportioning.—To conform with common practice, the ingredient materials of the mix were arbitrarily proportioned by volume for all tests. The only exception to this practice was the use of unit weights instead of the corresponding volumes, for the reason that greater uniformity was thus secured. The following mixes were used for the purposes indicated:

For all compression test cylinders used in the sand and the consistency tests, 1 : 2 : 4 and 1 : 2½ : 5;

For all reinforced-concrete beams used in the consistency tests, 1 : 2 : 4.

The standard Canadian bag of cement weighs 87.5 lb. net. The quantities of ingredient materials for a 1-bag batch of concrete were therefore:

	1:2:4.	1:2½:5.
Cement, lb	87.5	87.5
Sand, lb	186.5	233.4
Stone, lb	312.6	391.2
Water	As under heading "Mixing."	

Mixing.—The composition of a given test sand having been determined as already described, the quantities necessary for the sand aggregate of a batch of concrete were carefully weighed and placed in the mixer skip, raised, and deposited in the mixer drum. The incorporation of the portions of the original sands to form a uniform composite sand was produced by revolving the drum for a period of not less than five minutes. The "mixer-produced" sample for laboratory use was then taken from the drum and the remaining materials of the batch, except the water, were added in the ordinary way. The drum was permitted to make two revolutions or more before the water was added to the mix. Each batch was mixed one minute after all materials, including the water, had been deposited in the drum.

The mortar required to coat the inside of the mixer drum was provided by the addition of the necessary amount of sand, cement, and water to the first batch mixed each day. A test batch indicated that approximately 56 lb. of mortar were required for this purpose. The sand was, therefore, fixed at 50 lb. and the amounts of cement and water were varied to suit the mix. This amount was used for all sand tests and for the first, second and third consistency tests. For the fourth and fifth consistency tests the sand was reduced to 25 lb. to allow for the lesser quantity of mortar adhering to the drum.

As previously stated, sand No. 2 represents approximately an average of good quality sands for use in concrete available in the Toronto market. For this reason it was chosen as the sand to be first used in the tests for grading of sands. For the same reason it was used throughout the tests for consistency of mix.

The quantities of water used in the 1 : 2 : 4 mix and the 1 : 2½ : 5 mix were not predetermined. For the first batch of each mix three large pails of water were weighed before the mixing was begun; from these a sufficient amount of water was placed in the mixer to render the batch of the degree of wetness desired, and from the weight of the water remaining in the pails the amount placed in the mixer was determined. This quantity was used as a standard for all succeeding batches in the tests for grading of sands and for the first-consistency batches of the tests for consistency of mix.

Regarding the consistency, or degree of wetness, of the mix used as a standard, it may here be explained that it was the intention of the observer at the mixer to produce a concrete in which the stones were thoroughly covered with a thick coating of sticky, semi-plastic mortar, or in other words, a concrete of a consistency that, although saturated, shows no free water when taken out of the mixer; that can be transported in barrows, chutes, etc., without appreciable segregation of the component materials; and that when deposited in the forms will settle into place and become thoroughly compacted with a comparatively small amount of spading, slicing, or other manipulating.

Having determined the quantity of water for the first consistency of each mix, the quantities of water for four other consistencies were arbitrarily fixed by the percentages of increase for each, namely, 10 per cent for the second, 20 per cent for the third, 35 per cent for the fourth, and 50 per cent for the fifth.

For the tests for grading of sands, the quantity of water used, when expressed in relation to the total dry weight of the cement and aggregates and to the cement alone, is given in Table II.

TABLE II.—PERCENTAGE OF WATER USED IN TESTS FOR GRADING OF SANDS AND CONSISTENCY OF MIX.

Test.	Kind of Specimen.	1 : 2 : 4		1 : 2½ : 5	
		Percentage of Water to Dry Weight of		Percentage of Water to Dry Weight of	
		Cement and Aggregates.	Cement.	Cement and Aggregates.	Cement.
Grading of sands		6.17	41.14	5.99	48.76
Consistency:					
First	Beam	6.48	43.36	....	....
	Cylinder	6.17	41.14	5.99	48.76
Second	Beam	7.13	47.70	....	....
	Cylinder	6.75	45.25	6.59	53.64
Third	Beam	7.78	52.03	....	....
	Cylinder	7.37	49.37	7.19	58.51
Fourth	Beam	8.75	58.52	....	....
	Cylinder	8.28	55.54	8.09	65.83
Fifth	Beam	9.72	65.04	....	....
	Cylinder	9.26	61.71	8.99	73.14

Table II also gives the percentage of water used in the tests for consistency of mix.

In connection with the data given in Table II, it must be borne in mind that all concrete materials were thoroughly dry when deposited in the mixer drum.

The detail of the test beams rendered it necessary that the concrete of first consistency for this work be slightly more mushy than that for the test cylinders.

The quantity of concrete mixed for each set of test specimens was sufficient to fill all molds and to give a residue of from 1½ to 2 cu. ft. The last test specimens made each day were, therefore, in no sense composed of scrapings from the mixer. In this connection, it is of interest to note that, although a less quantity of materials was provided for coating the inside of the drum with mortar, the quantity of concrete left over from the very wet mixes was approximately double that left over from the first or standard consistency. This increase was mainly due to the increased volume of water used.

A steam-operated Foote mixer of ⅓-cu. yd. capacity was used for the mixing of all concrete.

Placing.—The concrete was removed from the mixer as required for placing in the cylinder and beam forms. Usually about 2 cu. ft. of concrete were removed at a time and deposited in a barrow, from which it was shoveled into the forms as required for puddling. Before each succeeding barrowful was taken out, the mixer drum was given two or more revolutions to overcome any segregation of the ingredients of the concrete remaining in the mixer due to the action of gravity.

No special provision was made to prevent evaporation losses in the mixer drum and in the barrow. The mixing and placing was carried on in the open air and the usual variations of temperature and humidity of atmosphere existed from day to day as the work progressed. The mixing of batches was done from 7.45 to 8.30 a. m. The maximum variation in temperature was therefore only 46° F. In this connection, the author desires to mention the valuable information contained in Bulletin No. 81, "The Influence of Temperature on the Strength of Concrete," University of Illinois Engineering Experiment Station.

In the puddling of the concrete in the forms, a special effort was made to do the work with the greatest dispatch, consistent with the securing of uniform results, special attention being given to thorough compacting and the uniform distribution of the broken stone and mortar throughout each cylinder and beam.

Bars having a diameter of $\frac{5}{16}$ to ⅜ in., flattened to form a comparatively thin blade-like end, were used for puddling. Compacting of the concrete and the exclusion of entrapped air was mainly accomplished by tapping the exterior surface of the forms with a hammer. In the consistency tests this latter method of compacting was used only in the preparation of cylinders and beams of the first and second consistencies. In the placing of concrete of the third, fourth and fifth consistencies the puddling bars were mainly used to uniformly distribute the stone aggregate.

All molds were filled at the mixing machine and were removed as soon as filled to the storage shed, a distance of about 100 ft. The top surfaces of cylinders and beams were here troweled slightly to remove irregularities.

Time of Mixing.—Information indicating the effect of varying the time of mixing was desired, and the specimens for this investigation were prepared as follows:

The ingredients necessary for a 1½-bag batch of 1 : 2½ : 5 concrete were placed in the mixer drum and a sufficient quantity of water to produce concrete of first consistency. The mixing was timed from the completion of placing all concrete materials in the mixer drum. When the mixing had continued ¼ minute a sufficient quantity of concrete was removed to mold the ¼-minute group of cylinders. As soon as these cylinders had been puddled, the mixing was continued an additional ¼ minute to mix the concrete for the ½-minute group of cylinders. In a similar manner the concrete was mixed for the 1 and 2-minute groups of cylinders.

The uncontrollable conditions of temperature, humidity, evaporation, etc., above mentioned constituted the main factors, of a foreign nature, tending to produce irregularities in the mixing and placing, the personal equation being reduced to a minimum by reason of the fact that the work was performed by the same men from day to day.

In a few cases it was found that the water content fixed upon in the first batch of each mix was insufficient to produce a plastic mortar. A greater amount of labor was required in such cases to produce a uniformly compacted mass.

Sands having little fine material, with a considerable proportion of coarse, were found to be unadapted to the production of a "sticky, semi-plastic mortar." The water content of such mortar sinks by gravitation below the surface and doubtless takes with it some of the cement, thus producing an uneven distribution of that important element of the mix.

In relation to the volume of cement (assumed at 100 lb. to produce 1 cu. ft. of cement paste), the quantity of sand used in each mix was greater than that used in common practice. As already shown, the shrinkage in volume due to thorough drying amounted to 18.7 per cent per cubic foot of wet sand.

To eliminate possible irregularities throughout the work, two men prepared the ingredients of the specially graded sands and apportioned the concrete materials for each batch. These men with two others mixed and placed the concrete. In general, all placing of concrete in the forms was the work of three individuals. The troweling of the top surfaces of cylinders and beams was done by one of these. The main variables, namely, the gradings of the sands and the consistency of mix, were thus as clearly defined as practicable.

Storage.—The forms as soon as filled were placed in a corrugated-iron covered shed.

In general, the forms were removed from the test specimens 24 hours after filling. The few exceptions to this rule were due to slow setting of the concrete, resulting from cold or wet weather. As soon as removed from the forms the test specimens were marked for identification and placed in beds of moist sand, where they remained until required for the compression or bend test.

The reinforced-concrete beams were handled with special care in order not to produce incipient fractures.

Mixing of concrete was commenced on March 30, 1916. It was, therefore, necessary to maintain artificial heat in the storage shed during the night and at times during the day for a period of from two to three weeks.

DESCRIPTION OF TESTS.

Tests of Mortar Briquettes and Cubes.—The tests of mortar briquettes and cubes were undertaken with the primary object of securing information relative to the mortar values of the 12 test sands. The secondary object of the tests was to discover the relation, if any, existing between the results obtained from the testing of 2-in. mortar cubes and those obtained from the testing of 6 by 12-in. cylinders. The mortars were composed of 1 part cement to 3 parts sand.

Standard briquettes and 2-in. cubes were prepared in the laboratory from the "mixer produced" samples of the test sands. The methods and equipment used for this work were in accord with the standards of the Society, and the mortars were of normal consistency. The work in the laboratory was entirely performed by one operator.

Tests of Cylinders and Beams.—The two series of tests upon concrete cylinders and beams were originally planned as follows:

For tests for grading of sand, the test specimens were cylinders, 6 in. in diameter by 12 in. long. Five specimens were tested at each age of 7, 30, 60, 90 days and 1 year.

For tests for consistency of mix, the test specimens were cylinders, 6 in. in diameter by 12 in. long, and reinforced-concrete beams, 4 by 6 in. in cross-section by 4 ft. long, reinforced as shown in Fig. 7. Five cylinders were tested at each age of 7, 30, 60, 90 days and 1 year, and five beams at each age of 30, 60 and 90 days.

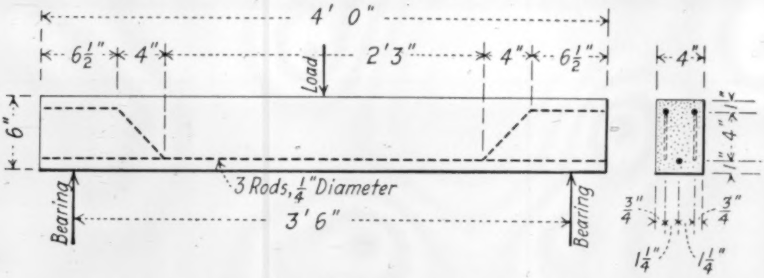


FIG. 7.—Details of Reinforced-Concrete Beam used in Tests.

The Olsen testing machine of 100,000 lb. capacity, owned by the City of Toronto, was used for practically all cylinder tests, except those in which the tests of the previous age showed an average strength per cylinder of over 80,000 lb.

The Riehle machine of 200,000-lb. capacity, owned by the University of Toronto, was used whenever it seemed probable that the strength of the cylinders would exceed the capacity of the smaller machine. All tests of beams were made upon the 100,000-lb. capacity machine. The vertical movement of the

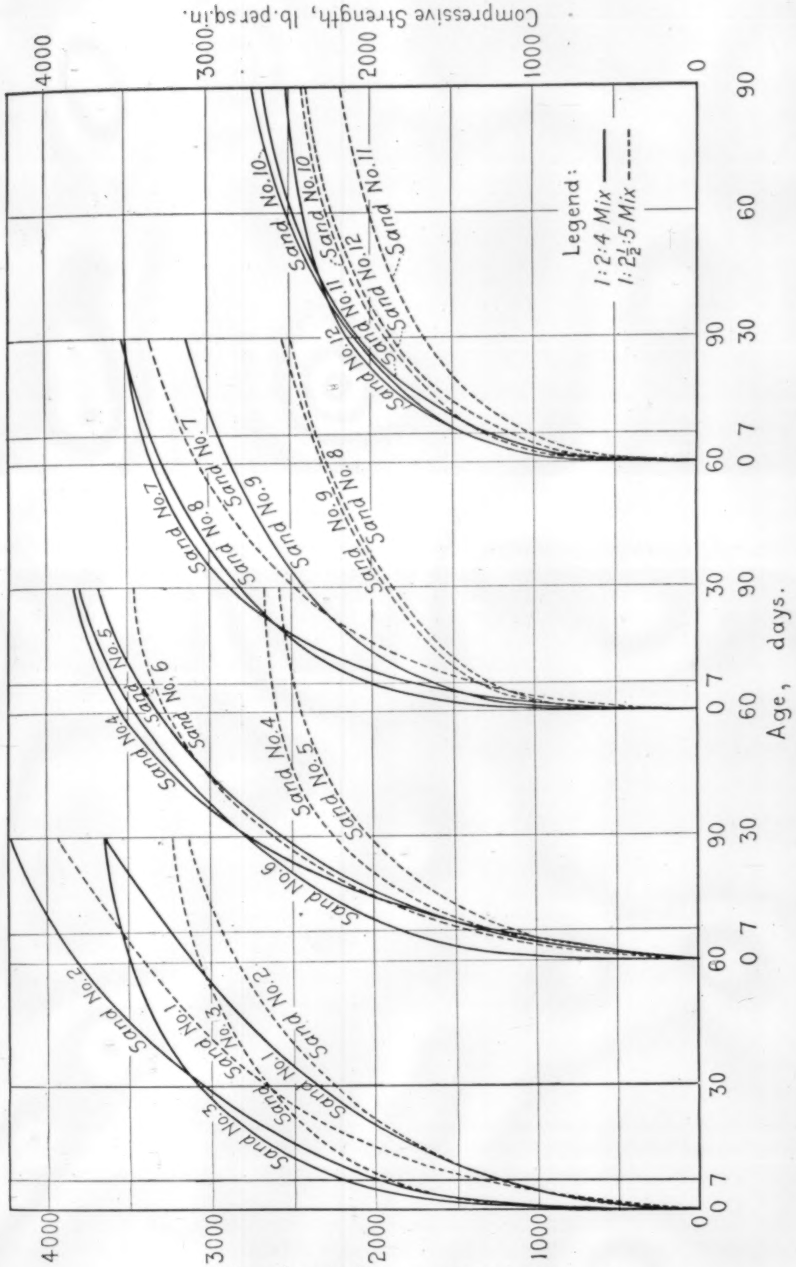


FIG. 8.—Compressive Strengths of Test Cylinders.

Olsen machine was about 0.016 in. per minute, while that of the Riehle machine was about 0.02 in. per minute.

The edges of all test cylinders were slightly rounded by grinding them with a carborundum brick before testing. When placed in the testing machine the ends of the cylinders were bedded with sheets of heavy beaver board, having a thickness of $\frac{7}{32}$ in., to secure an even distribution of the load. It is believed that this use of beaver board gave quite as consistent and accurate results as would have been secured with plaster of Paris or similar material. A considerable saving of time was effected in this way.

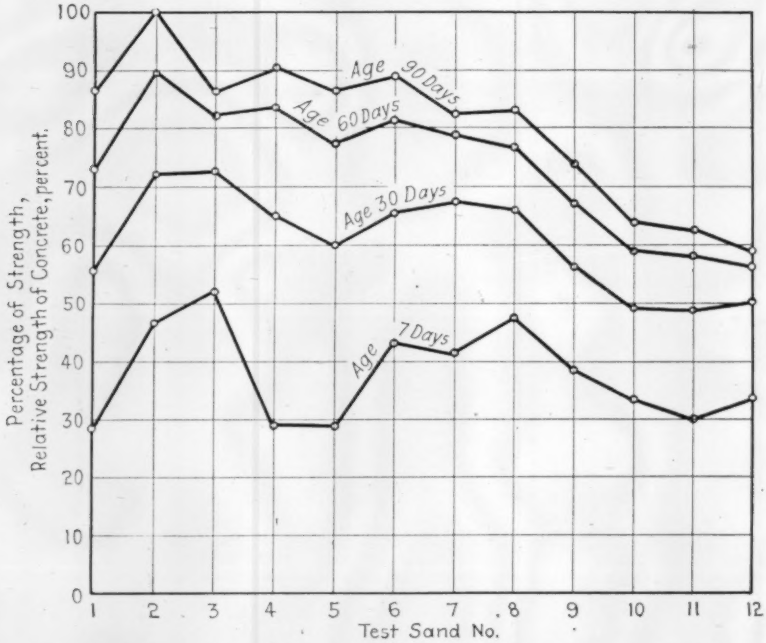


FIG. 9.—Relative Compressive Strengths of Test Cylinders, 1 : 2 : 4 Mix. Based on Compressive Strength of Sand No. 2 at 90 days.

In all cylinder tests a spherical-seated bearing block was used.

All beams were tested with a concentrated load at the center. The end bearings and center block through which the load was applied were semi-cylindrical in shape. The bearings upon the concrete were padded with narrow strips of beaver board.

For all tests of cylinders, the maximum load at ultimate failure was recorded. For all tests of beams, the load recorded was that judged to have caused the failure of bond between the steel and concrete. This so-called "critical" failure, especially in beams of the fourth and fifth consistencies, was more or less difficult to detect, since it was shortly afterward followed by the sustaining of an increased load due to the mechanical bond produced by the bent bars.

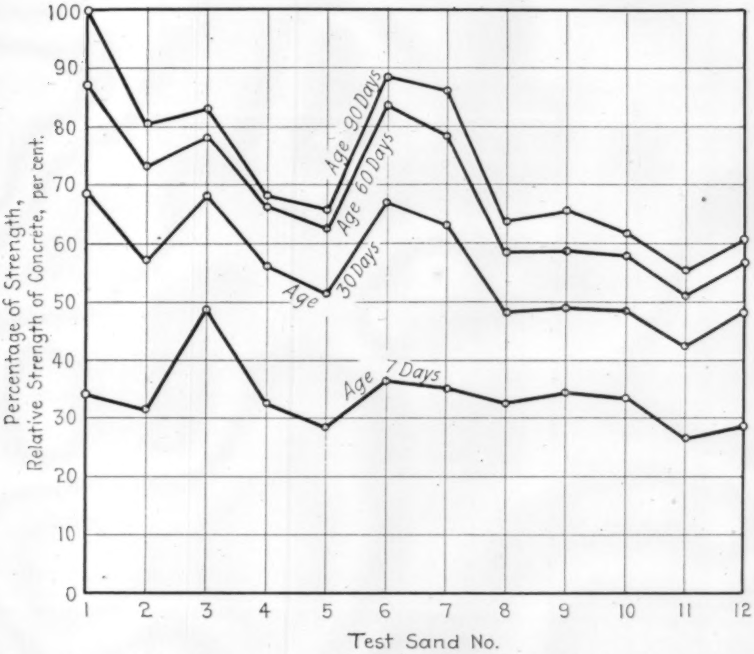


FIG. 10.—Relative Compressive Strengths of Test Cylinders, 1 : $2\frac{1}{2}$: 5 Mix. Based on Compressive Strength of Sand No. 1 at 90 days.

During the period of making tests the Riehle machine was twice disabled in connection with other tests. The time involved in the securing of repair parts, together with the testing of accumulated materials mainly for war purposes, disarranged to a considerable extent the time schedule of the cylinder tests.

RESULTS OF TESTS.

The unavoidable "overdue" tests just referred to, rendered it advisable that the results obtained be reduced to graphical form, in order to permit of the making of direct comparisons. The results of all tests are, therefore, shown graphically rather than in tabulated form.

Tests for Grading of Sands.—Fig. 8 shows the compressive strengths obtained from the tests of the cylinders in which the specially graded sands were used. The rather inconsistent condition shown, wherein sand No. 1 gives greater strength for a 1 : 2½ : 5 mix than for a 1 : 2 : 4 mix, is not easily explained. No errors were found in the proportioning of the mix. The cylinders for both mixes were made early in April, 1916, so it is possible that conditions of temperature may have materially influenced results. The grading of the sand is favorable to a high-strength 1 : 2½ : 5 concrete. It should be noted that the strengths given for sand No. 5, 1 : 2½ : 5 mix, are slightly in error due to a mistake having been made in the composition of

TABLE III.—ACTUAL GRADINGS OF SANDS.
SAMPLE FROM MIXER.

Sieve No.	Percentage retained on Sieve.											
	Sand No.											
	1	2	3	4	5	6	7	8	9	10	11	12
¼ in.	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
8	8.5	15.0	17.0	11.5	15.5	23.0	14.0	19.0	17.5	18.5	19.0	20.0
10	8.0	5.5	8.0	6.0	3.5	6.0	4.0	3.0	10.0	6.0	6.0	4.0
20	20.0	28.5	32.0	22.0	25.5	30.0	23.0	23.5	23.0	30.0	30.0	31.0
30	14.0	11.5	13.0	20.0	19.0	12.0	13.0	15.0	14.5	9.0	12.0	13.0
40	15.0	11.5	12.0	17.0	13.0	13.0	15.0	14.5	10.5	10.0	12.5	14.5
50	14.5	12.0	10.0	6.0	4.5	3.5	15.5	13.5	18.0	11.0	10.0	12.0
80	15.0	12.5	5.5	12.5	10.0	8.0	10.5	9.0	5.5	11.0	6.5	4.0
100	3.0	2.0	1.5	2.5	3.0	2.5	3.0	2.0	1.0	3.0	2.0	1.0
100 (passing)	2.0	2.0	1.0	2.0	2.0	2.0	2.0	1.5	0.5	2.0	2.0	0.5
Total	100.0	100.5	100.0	99.5	99.5	100.0	100.0	101.0	100.5	100.5	100.0	100.0
Voids	30.0	29.60	30.06	30.6	31.0	29.10	29.10	29.26	30.53	27.66	27.51	30.2

the sand entering into the mix. This error was discovered after the mixing of concrete had been discontinued. With this exception, Table III gives the actual gradings for the sands of Fig. 8. Figs. 9 and 10, deduced from values taken from the curves shown in Fig. 8, show by percentages the relative strengths of the concretes.

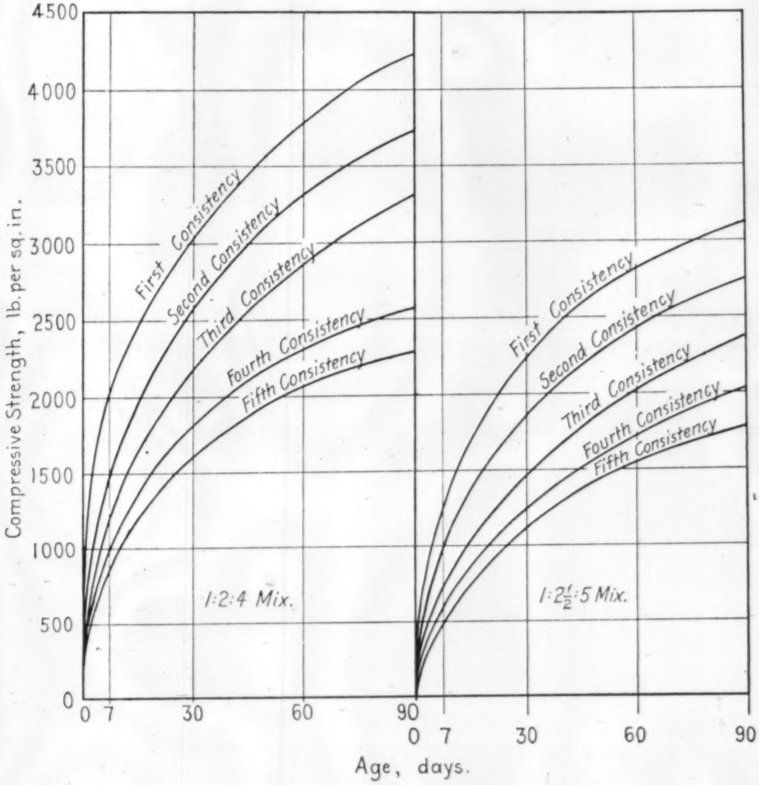


FIG. 11.—Compressive Strengths of Test Cylinders of Different Consistencies.

Tests for Consistency of Mix.—Fig. 11 shows the compressive strengths obtained from the tests of the cylinders, in which the consistency of the mix was varied from a sticky, semi-plastic to a very wet condition. The different percentages of water used in mixing the concrete for these cylinders are shown in Table II. Sand and stone were of limestone origin. The proportions are by volume, reduced to a unit weight basis; 1 cu. ft. of cement assumed to weigh 100 lb.

Fig. 12 was deduced from values taken from the curves shown in Fig. 11. It shows in a general way, by percentages, the relation between the strength of concrete of first consistency seasoned 90 days, and the strength of concretes of varying consistencies and ages.

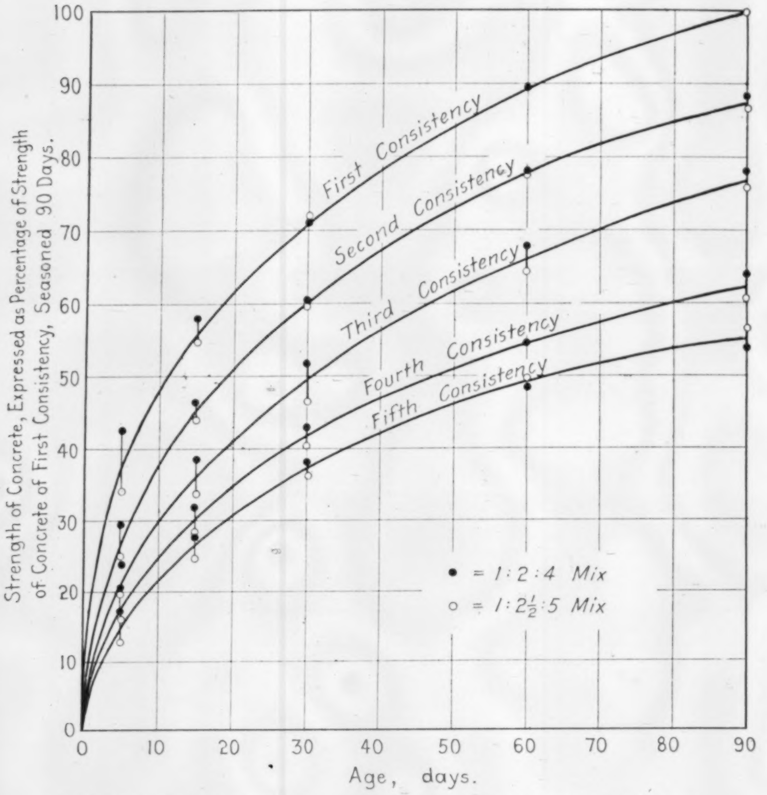


FIG. 12.—Relation between the Strengths of Concrete of Different Consistencies and Ages. Deduced from Fig. 11.

Fig. 13 was prepared from the results obtained in a series of compressometer tests upon cylinders of the first, third and

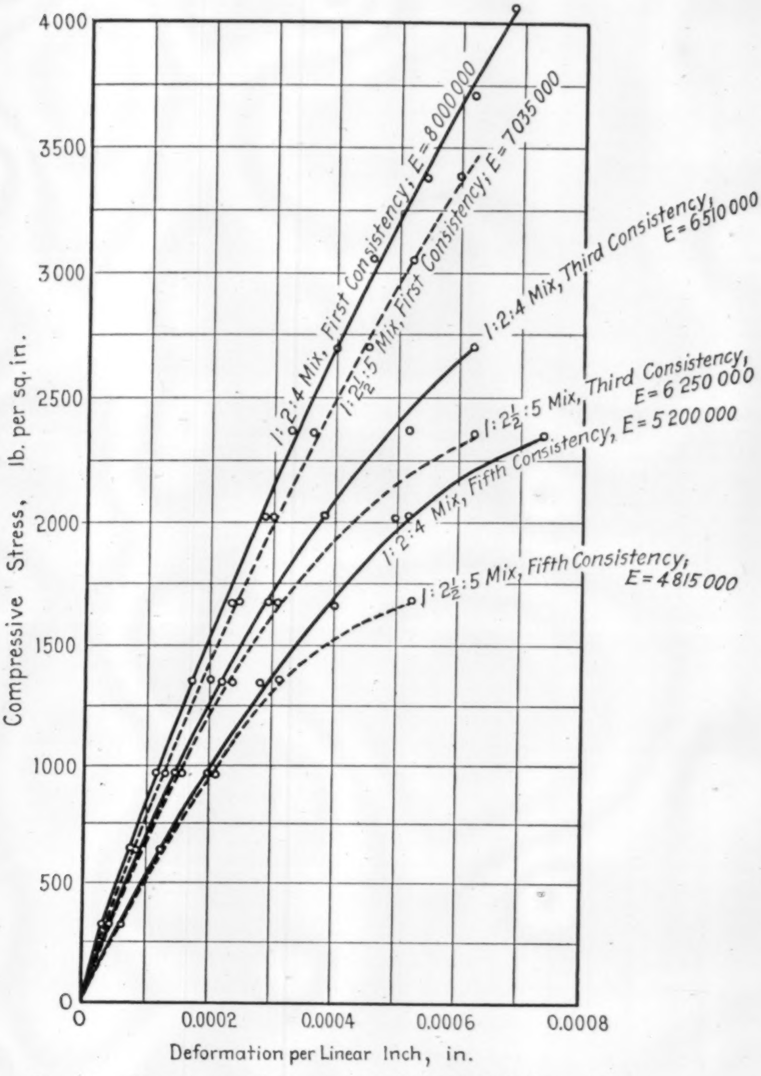


FIG. 13.—Compressive Tests upon Individual Cylinders of Concretes of Varying Consistencies.

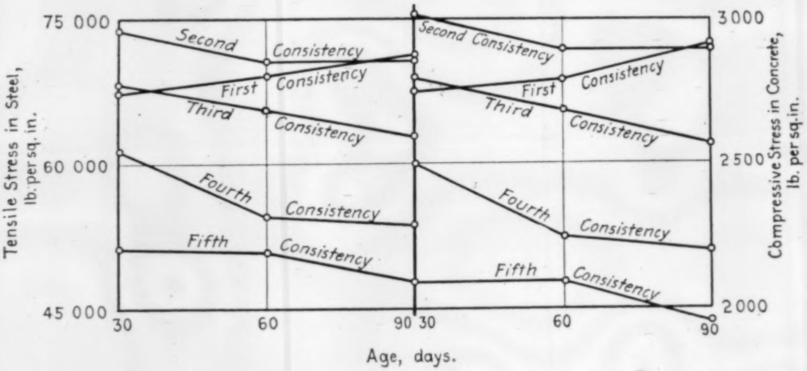


FIG. 14.—Stresses in Reinforced Concrete Beams at Various Ages; 1 : 2 : 4 Mix.

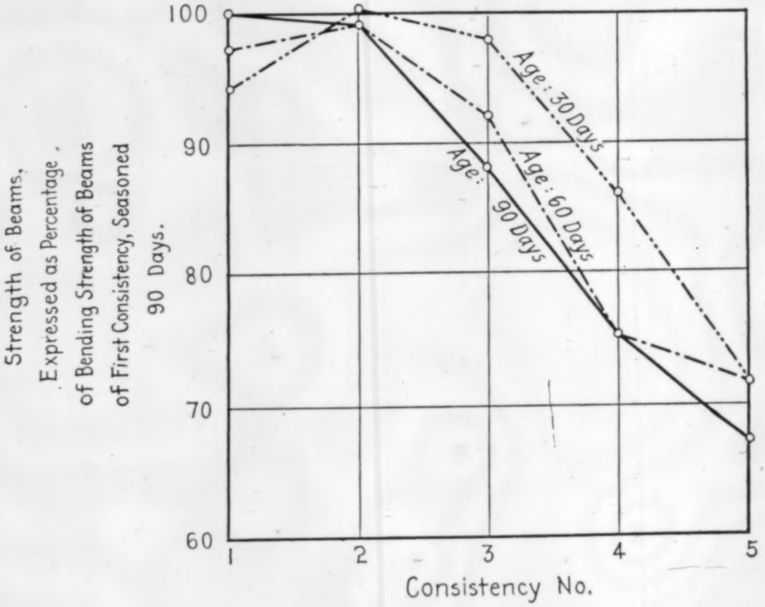


FIG. 15.—Relation between the Strengths of Beams of Various Consistencies and Ages. Deduced from Fig. 14.

fifth consistencies. Since the curves apply to individual cylinder tests they must be considered as indicating only the variability of the modulus of elasticity as affected by the water content of the concrete mix, rather than definite and reliable limits of such variability. The latter could only be determined from an average of the results obtained by testing five or more cylinders of each mix and consistency. The ages of the cylinders varied from 317 to 325 days. A reduction in the modulus of elasticity of concrete of the first consistency of approximately 35 per cent is shown in concrete of the fifth consistency.

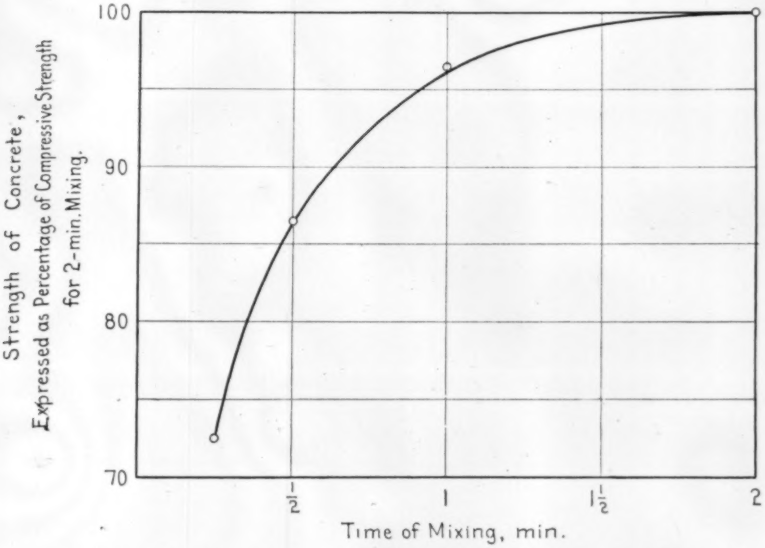


FIG. 16.—Relation of Time of Mixing to Compressive Strength of Concrete.

Fig. 14 shows the tensile and compressive strengths of the steel and concrete, respectively, as determined from the loads producing critical failure of reinforced-concrete beams, in which the consistency of the mix was varied from a rather wet, plastic condition to a very wet condition. In the determination of these stresses, the tensile stress borne by the concrete was disregarded in all cases.

Fig. 15 was deduced from Fig. 14. It shows in a general way, by percentages, the relation between the strength of reinforced-concrete beams of the first consistency, seasoned 90 days, and the strengths of beams of varying consistencies and ages.

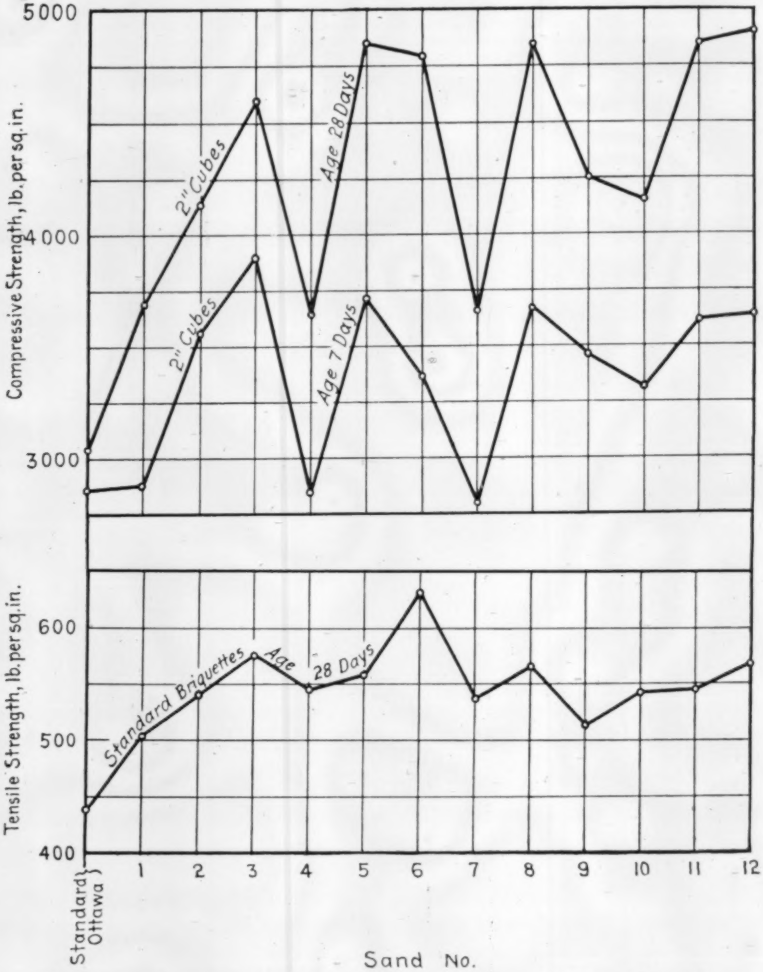


FIG. 17.—Compressive Strengths of 2-in. Mortar Cubes and Tensile Strengths of Mortar Briquettes; 1 : 3 Mix.

In the preparation of all specimens for consistency-of-mix tests, specially graded sand No. 2 was used.

Time-of-Mixing Test.—The cylinders composing the four groups were tested at the age of 162 days. From the results of this test the curve shown in Fig. 16 was prepared.

The abrupt change in the direction of the curve at the location indicating the 1 to 2-minute period of mixing, together with the rapid increase of strength shown for mixing periods of less than 1-minute duration, show conclusively the advantage gained by continuing the mixing operation for a period of from 1 to 2 minutes after all the materials have been placed in the mixer.

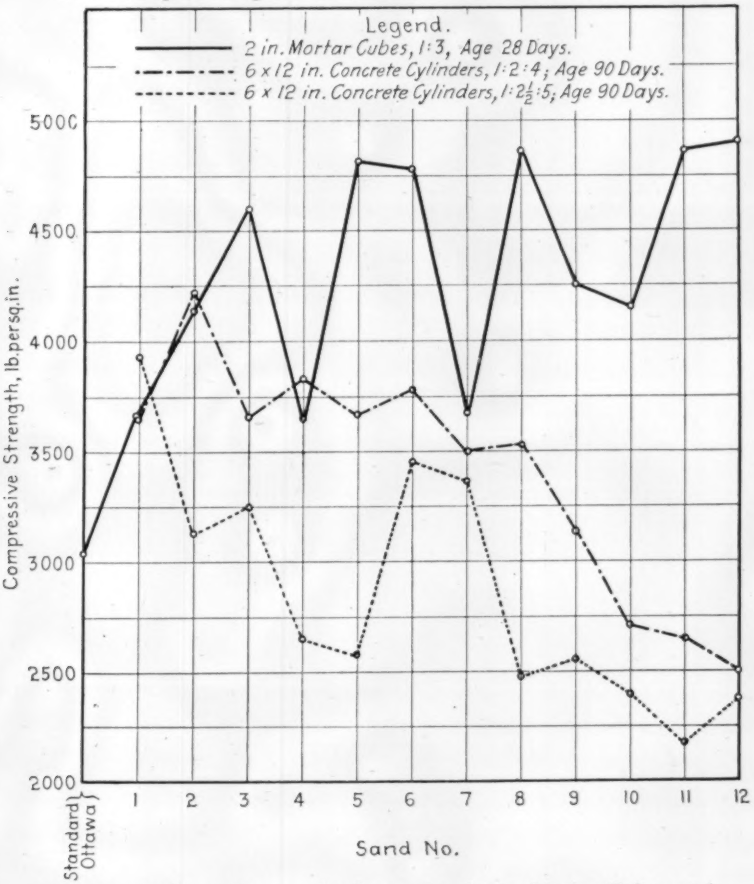


FIG. 18.—Relative Compressive Strengths of 2-in. Mortar Cubes, and 6 by 12-in. Concrete Cylinders.

Mortar Tests.—Fig. 17 shows the compressive strengths obtained from the tests of the 2-in. cubes and the tensile strengths of 1 : 3 briquettes. Each value is an average of the values of five specimens.

Fig. 18 shows the relative strengths of 2-in. mortar cubes, age 28 days, and 6 by 12-in. cylinders, age 90 days. The dissimilarity of the curves is of special interest.

OBSERVED PHENOMENA OF TESTS.

Failure of Cylinders.—In the tests of cylinders three forms of failure were observed, as follows:

1. Double cone, resulting from a thorough, uniform distribution of materials;

2. Diagonal shearing, resulting from the distribution of stone in a manner producing cleavage planes. Occasionally a wedge-shaped stone may be so placed as to cause a double-wedge-like fracture; and

3. A general "mushing down" or disintegration of the entire structure at its mid-length. This form of failure invariably indicates a failure of the mortar content of the concrete.

Although the three forms of failure were more or less common to all tests for grading of sands, it was found that the third was more generally characteristic of low-strength concretes. In these cases few of the stones were broken.

In the tests for consistency of mix, the first and second were the forms of failure for the first and second consistencies, while the third was the general form of failure for the fourth and fifth consistencies.

Fig. 19 shows a collection of broken cylinders indicating different forms of failure.

Failure of Beams.—Critical failure of beams of first and second consistencies was usually accompanied by the appearance of several vertical cracks, within the middle-third length of the beam. A further application of the load caused one of these cracks to develop, and ultimate failure was produced by the breaking of the two outer (bent) bars of the reinforcement and the slipping of the middle bar. Compression in the concrete caused little or no failure of same by crushing.

Critical failure of beams of the third consistency was similar to those of the first and second consistencies, except that crushing of concrete adjacent to the point of application of the load was more common.

Critical failure of beams of the fourth and fifth consistencies was accompanied by the appearance of a less number of vertical cracks. Usually two of these cracks developed simultaneously for a time, producing a wedge-like block in the center of the beam. Ultimate failure of beams of the fourth consistency was commonly produced by the breaking and slipping of the reinforcement on one side of the wedge, but in some cases was the same as described below for beams of the fifth consistency. Ultimate failure of these latter beams was produced by a spalling off of the concrete below the reinforcement at the center of the beam. In beams of both these consistencies, compressive stresses produced a distinct crushing of concrete adjacent to the point of application of the load.

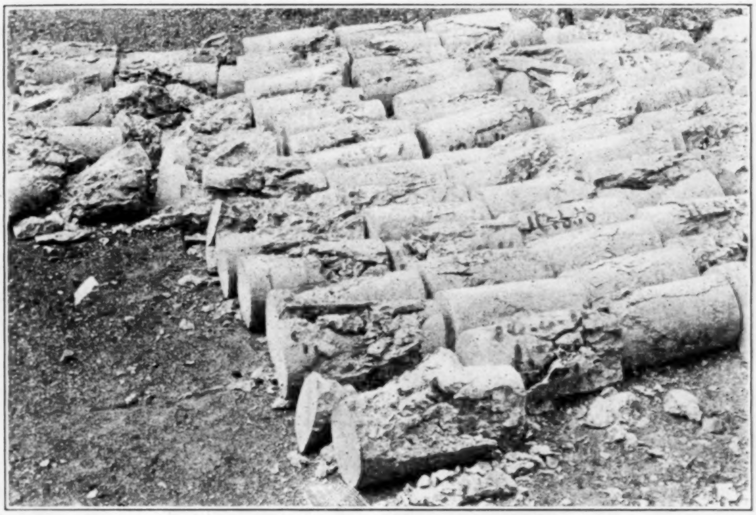


FIG. 19.—Broken Test Cylinders.

Fig. 20 shows typical forms of failure for the beams.

Relation of Weight of Test Cylinders to Strength.—In the tests for gradings of sands, all cylinders for 90-day tests were weighed before testing. This weight varied from 31 lb. 2 oz., minimum, to 32 lb. 3 oz., maximum, the average weight being about 31 lb. 7 oz. A critical examination of results shows no conclusive evidence that high strength and high weight are coincident. However, it is reasonable to assume that this should be the case.

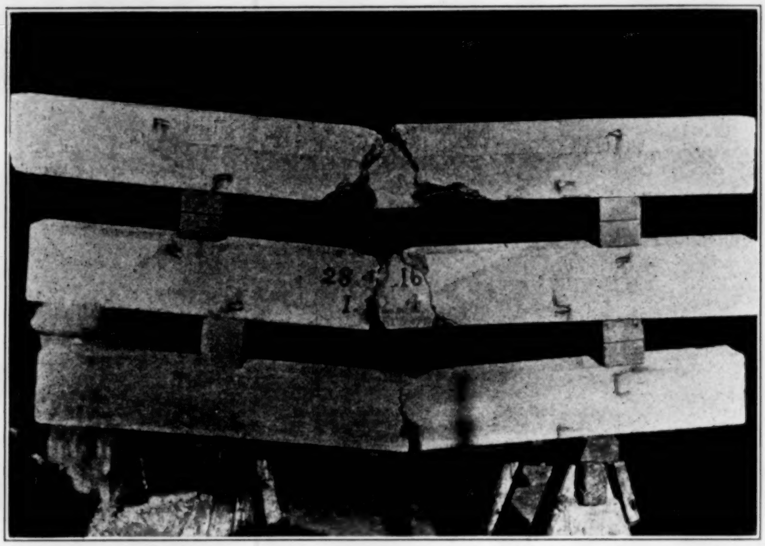


FIG. 20.—Typical Forms of Failure of Reinforced-Concrete Beams.

Texture of Concretes used for Testing Gradings of Sands.—The consistency of the mix used, together with the special care exercised in the mixing and placing of the concrete, insured a uniform distribution of the stone aggregate. Fig. 21 shows a cylinder selected at random sawed through the center. In addition to the distribution of the stone, the wedging action of the stones is shown. Fig. 22 shows a rather unique combination of stones in a cylinder cone. This cylinder broke under a load 37,072 lb. below the average of the other four cylinders composing the set of five. Single stones and combinations of stones tending to produce cleavage planes are unavoidable.

The texture of the mortar content for a given mix varies very considerably with the grading of the sand. Fig. 23 shows the texture of the mortar in 1 : 2 : 4 concretes of sands Nos. 2 and 12. The granular appearance of the latter is largely due to the inadequacy of the fine material of the sand to bolster-up the cement by the formation of a sandy paste.

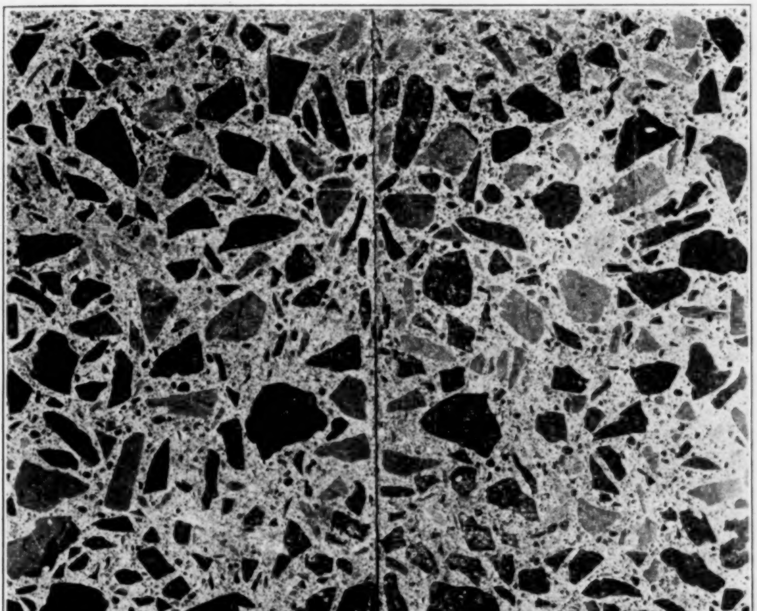


FIG. 21.—Distribution of Aggregate in Concrete, 1 : 2 : 4 Mix.

Irregularity in the strength of individual cylinders at the age of 30 days or over indicates to some extent the unreliability of the sand for use in a concrete intended to resist impact. This condition is not evident from a study of average strengths.

The characteristic failure for high-strength concretes, age 30 to 90 days, was a sudden break usually accompanied by a loud report. For low and for irregular-strength concretes, the methods of failure varied from a sudden break to a "mush down."

Toughness and durability were roughly tested under the impact of hammer blows. Friability and shortness so observed were not confined to low-strength concretes. However, this condition was common to all sands producing concretes of low strengths and to all consistencies of mix below the second.

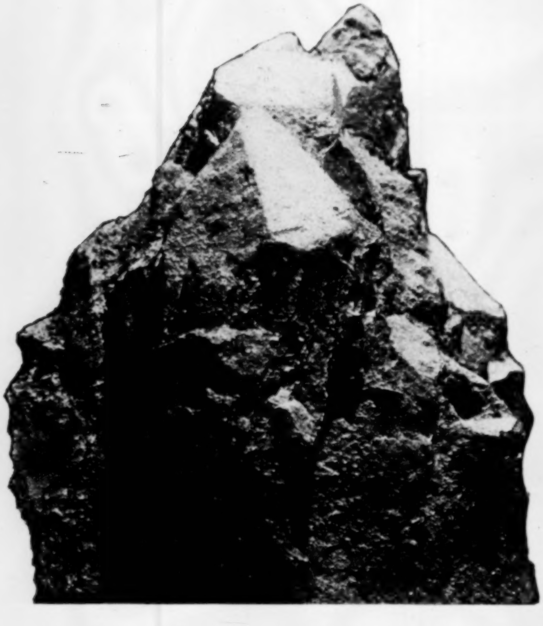
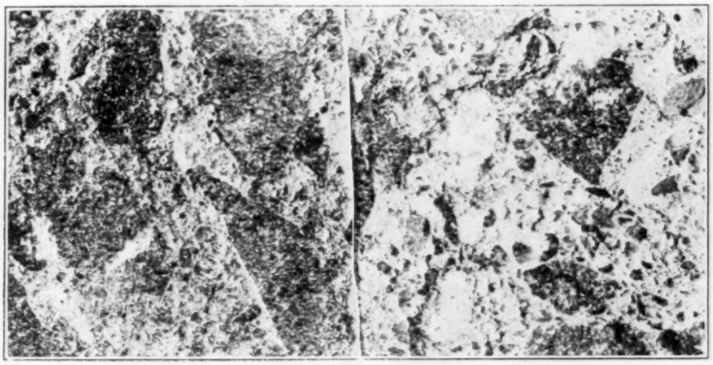


FIG. 22.—Specimen of Low-Strength Cylinder; due to Distribution of Stone Aggregate.

Air and Water Voids and Cavities.—In the following discussion the author uses the word "void" to indicate a spherical shaped opening within the body of the mortar content of the concrete. The word "cavity" is used to indicate an opening, usually irregular in shape, located adjacent to the surface of a particle of the sand or the stone aggregate.

The semi-plastic mix described under "mixing" is well adapted to the removal of occluded air, when a reasonable amount of work is done in connection with the compacting of the concrete during placing. The consistency of the mix is such that excess water is not available for the production of myriads of water voids and cavities. However, it is not possible to produce an absolutely voidless concrete.

Fig. 24 shows the character of concrete produced as above described. This photograph, taken in connection with field construction work, shows a very small number of voids in the mortar content and the absence of cavities adjacent to the stone particles. There are also fractured sand grains and stone particles, indicating beyond question that the mortar content of this concrete is equally as strong as the stone aggregate.



(*a*) Sand No. 2. (*b*) Sand No. 12.

FIG. 23.—Texture of Mortars in Concrete, 1 : 2 : 4 Mix (×2½).

Present-day methods of preparing, transporting, and placing concrete are especially favorable to the occlusion of air in the concrete produced. This air becomes entrapped in the concrete mass forming voids and cavities. In a well-prepared and properly placed concrete the actual volume of the voids and cavities produced by occluded air is relatively small, as compared with that produced by the water content of the mix. In a concrete of sloppy consistency, the voids and cavities produced by air are, no doubt, of less volume than in a saturated semi-plastic mix, since the less resistance of the former permits air to more readily escape.

Unlike air, the inclusion of water as a constituent material of concrete is absolutely necessary. Its proper function is (1) to combine with the cement in producing the pasty cementing material necessary for the union of the aggregates into a thoroughly united, uniformly strong, conglomerate mass, and (2) to render this mass, in its transitory state, capable of being formed with a reasonable amount of work into the various shapes required in its adaptation to the construction of bridges, buildings, roadways, and various other practical uses.

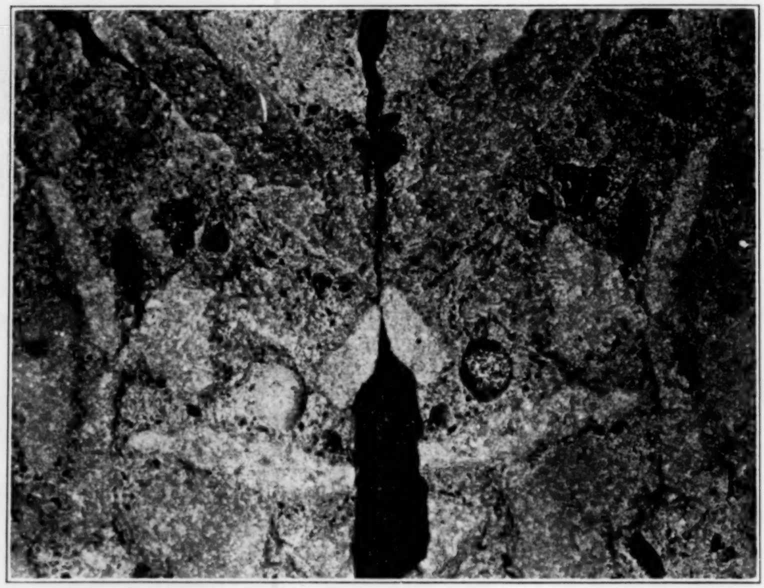
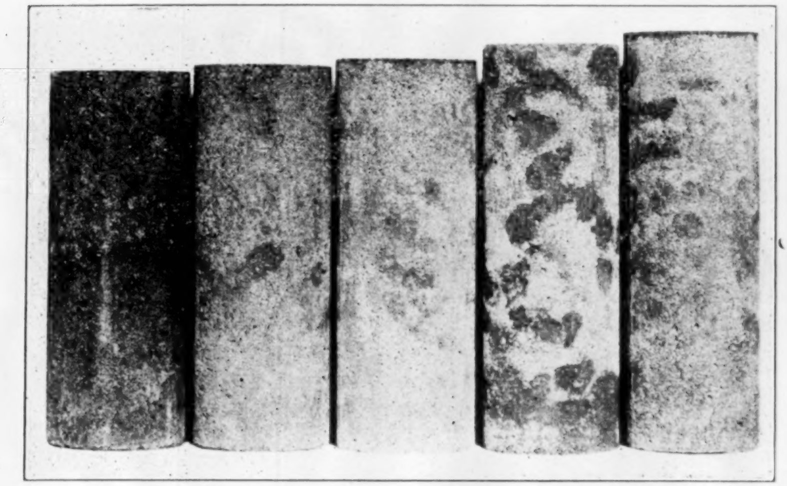


FIG. 24.—Field Produced Concrete of Limestone Aggregates; 1 : 2 : 4 Mix (×1½).

The addition of a small quantity of water to a saturated, semi-plastic concrete mix, will change its consistency to that of a readily flowing mixture. The inclusion of a greater quantity of water than is required for the development of its natural functions, is decidedly detrimental to the strength and reliability of the final concrete.

Fig. 25 shows a series of mortar cylinders in which the quantity of cement and sand is constant. The water content was successively increased by 10, 20, 35 and 50 per cent over that required to produce normal consistency. The bulking-up effect of the water is apparent.



(*a*) (*b*) (*c*) (*d*) (*e*)

(*a*) Original. (*b*) 10 per cent. (*c*) 20 per cent.
(*d*) 35 per cent. (*e*) 50 per cent.

FIG. 25.—Mortar Cylinders in which the Water Content was increased by 10, 20, 35, and 50 per cent.

The effect of increasing the water content from the quantity used in concrete of the so-called standard or first consistency, to that of concrete of the fifth consistency, is clearly shown in Figs. 26 to 31, inclusive, for cylinders, and in Figs. 32 to 35 for reinforced-concrete beams.

Fig. 26 shows a 1 : 2 : 4 concrete of first consistency produced from sand No. 2. Voids are minimum. The cylinder from which the specimen was taken was tested at an age of 308

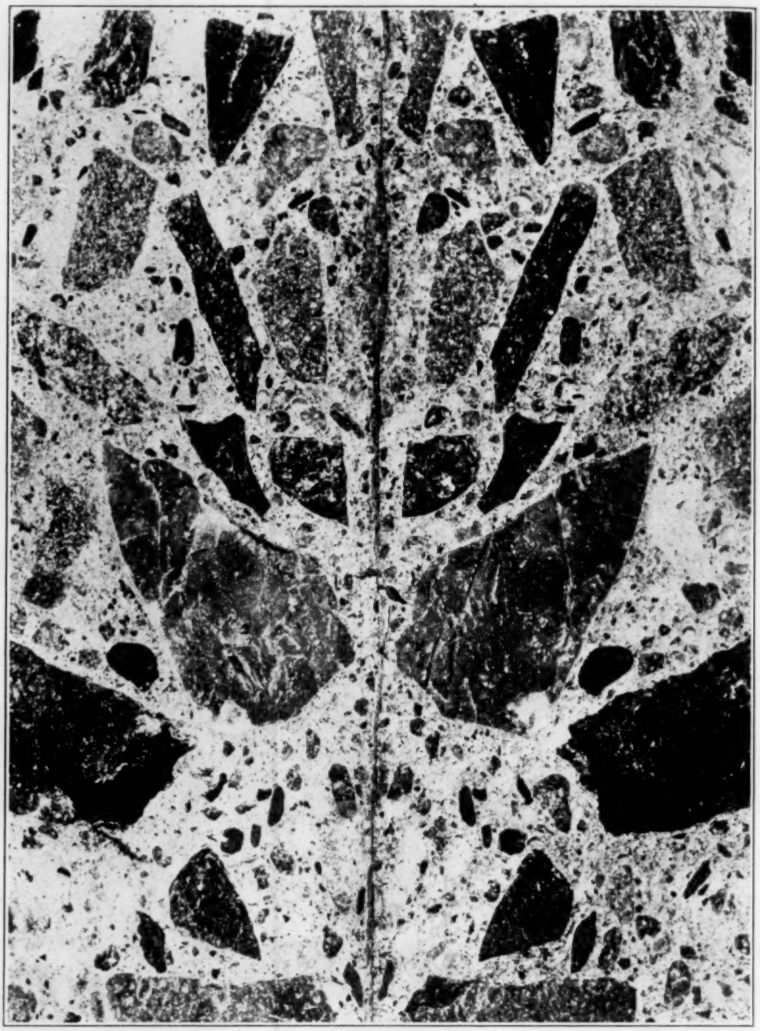


FIG. 26.—Fractured Concrete; 1 : 2 : 4 Mix; First Consistency (×1½).

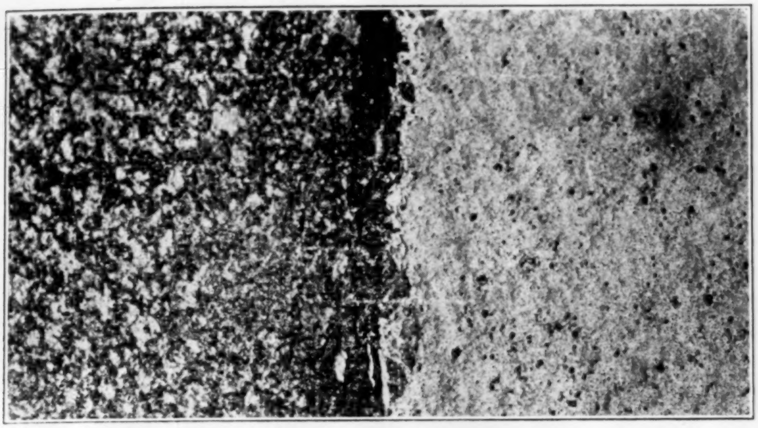


FIG. 27.—Surface Contact between Under Side of Stone and Mortar Bed; 1 : 2 : 4 Mix; First Consistency (×5½).

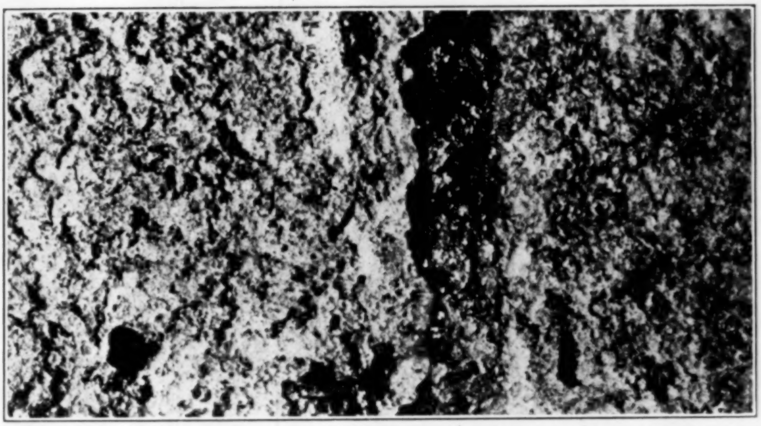


FIG. 28.—Surface Contact between Under Side of Stone and Mortar Bed; 1 : 2 : 4 Mix; Fifth Consistency (× 5½). Note Voids and Cavities due to Excess Water.

days. Its compressive strength was 168,970 lb. (5735 lb. per sq. in.).

Fig. 27 shows a 1 : 2 : 4 concrete of first consistency. The under surface of the stone was in close contact with its mortar bed, and is slightly coated with a gray film of cementing material. The mortar bed is grayish in color, showing few voids and no cavities.

In the test cylinders of concretes of second consistency, three conditions were noticeable, namely, (1) an increase in the number of voids in the mortar bedding of the stones; (2) a tendency of the concrete toward friability and shortness; and (3) a general tendency of the concrete to become white in color, as compared with the gray color of concrete of the first consistency.

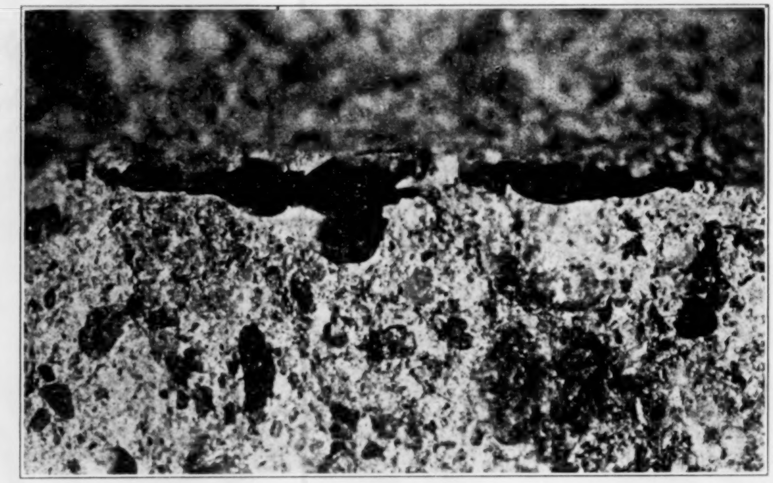


FIG. 29.—Water Cavities Underlying Stone Aggregate (X 8).

Fig. 28 shows a 1 : 2 : 4 concrete of fifth consistency. As compared with concrete of first consistency, Figs. 26 and 27, which contain a comparatively small volume of voids in the mortar and no cavities adjacent to the stones, it is evident that the voids have not only increased in number and size, but that the excess water, in the physical and chemical operations inci-

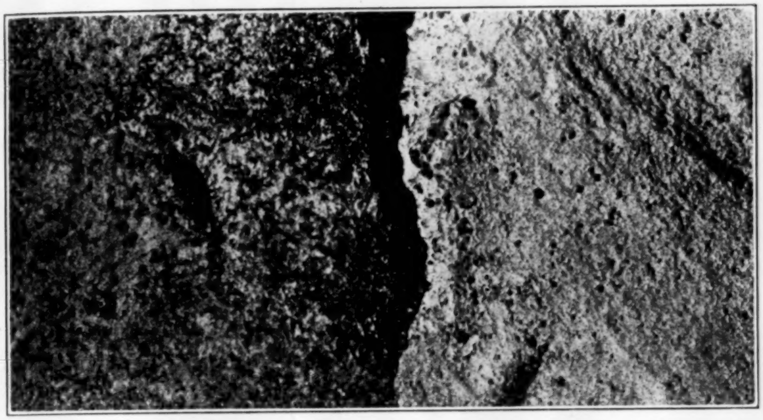


FIG. 30.—Surface Contact between Under Side of Stone and Mortar Bed; 1 : 2½ : 5 Mix; First Consistency (× 5).

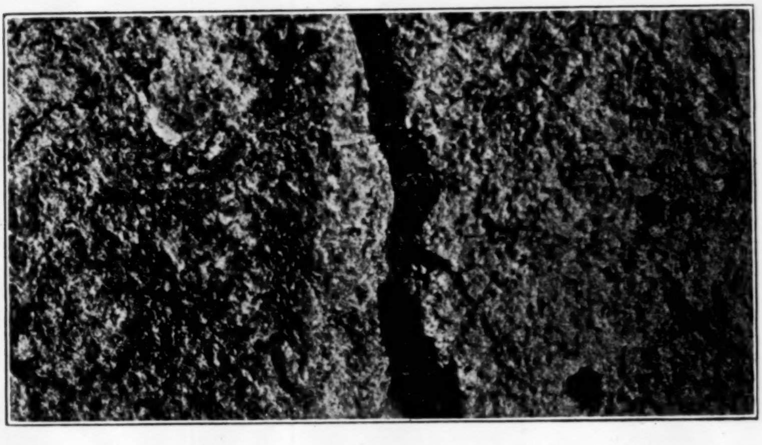


FIG. 31.—Surface Contact between Under Side of Stone and Mortar Bed; 1 : 2½ : 5 Mix; Fifth Consistency (×5). Note Voids and Cavities, also "Valleys" in "Laitance" on Surface of Stone.

dent to the mixing, placing and hardening of the mass, has accumulated in flattened globules adjacent to the stones, and by its final evaporation, has left comparatively large cavities which are separated by ridges of a soft, chalk-like substance upon which the superimposed stones must take their bearing. The under surface of the stone is also covered with this material—"laitance"—giving to it a pitted, crater-covered appearance, not unlike that of the surface of the moon when seen through a good field glass. A similar appearance is occasionally found upon the surface of water-cooled furnace slag.

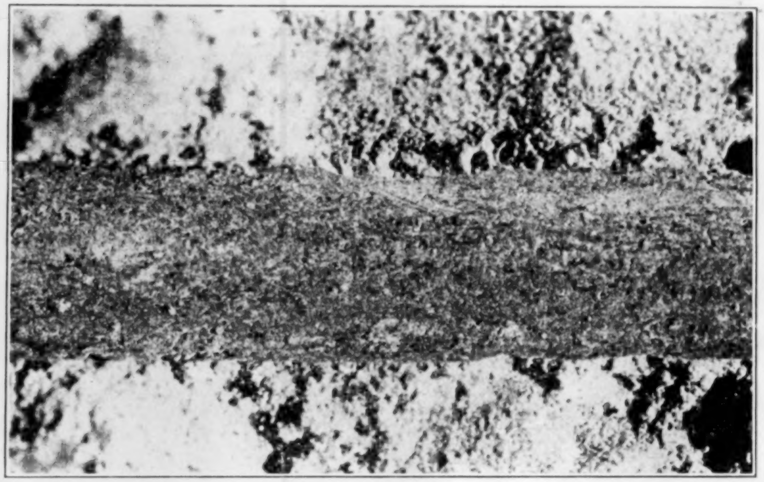


FIG. 32.—Texture of Mortar forming Bond on Upper Side of Reinforcing Steel, Bar Removed; 1 : 2 : 4 Mix; First Consistency (× 4).

The accumulation of water cavities adjacent to the stone aggregate, together with the formation of the dividing walls or ridges composed mainly of lime which separate them, probably constitutes the most important reason why concrete of the fifth consistency attains a strength of approximately 55 per cent of that attained by concrete of first consistency. The increased volume of voids is also an important factor to the same end.

In this connection, the water cavity shown in Fig. 29 is of special interest.

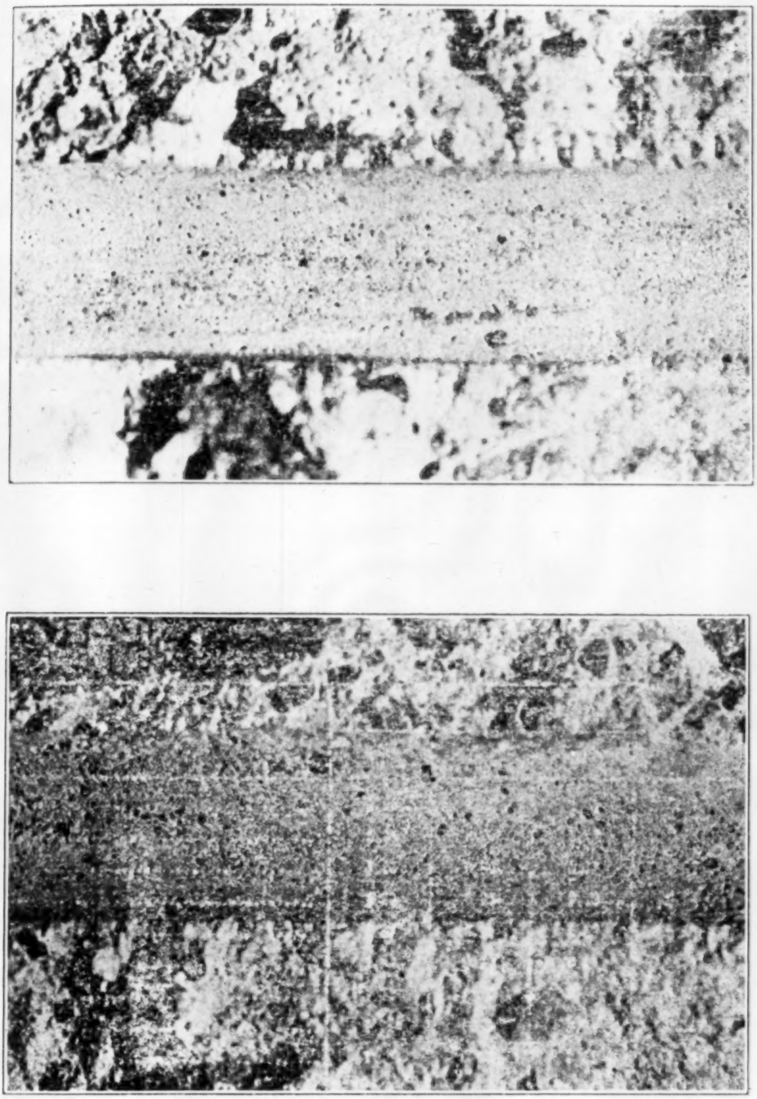


FIG. 33.—Texture of Mortar forming Bond on Under Side of Reinforcing Steel, Bar Removed; 1 : 2 : 4 Mix; First Consistency (× 4).

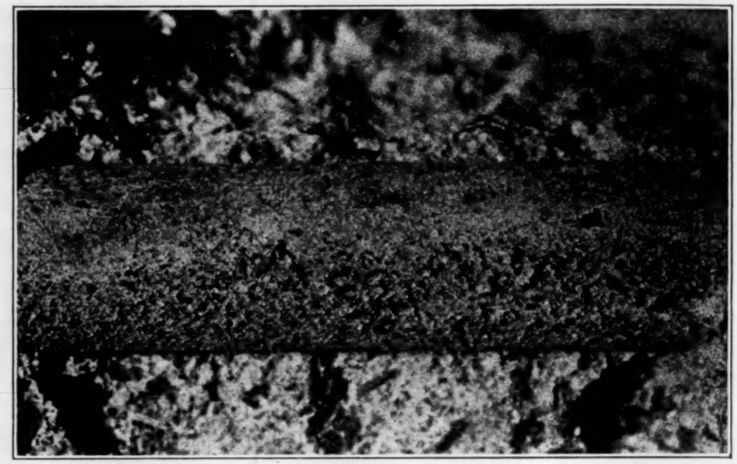


FIG. 34.—Texture of Mortar forming Bond on Upper and Under Sides of Reinforcing Steel, Bar Removed; 1 : 2 : 4 Mix; Fifth Consistency (× 4).

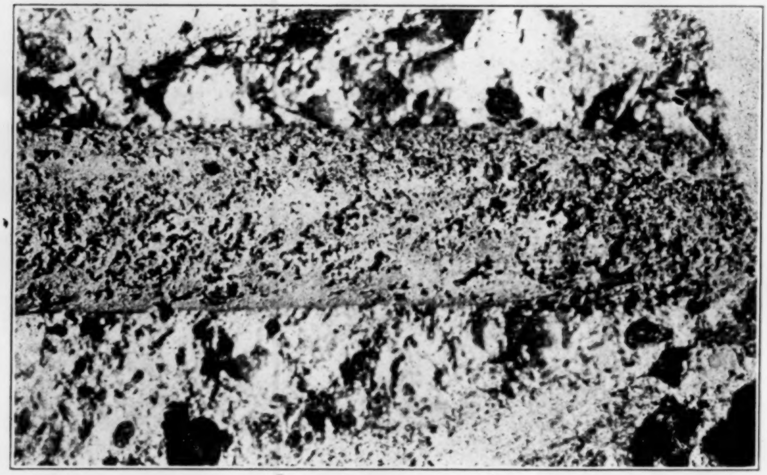


FIG. 35.—Texture of Mortar forming Bond on Under Side of Reinforcing Steel, Bar Removed; 1 : 2 : 4 Mix; Fifth Consistency (× 4).

Figs. 30 and 31 show for 1 : $2\frac{1}{2}$: 5 concretes of the first and fifth consistencies, respectively, the condition of the under surfaces of stones and of the mortar beds under same. The remarks made above relative to 1 : 2 : 4 concretes of first and fifth consistencies (Figs. 27 and 28) apply here, with the exception that in the case of Fig. 31 the irregular lines upon the mortar bed were apparently produced by currents of water, which washed away a portion of the accumulated laitance, leaving miniature valleys.

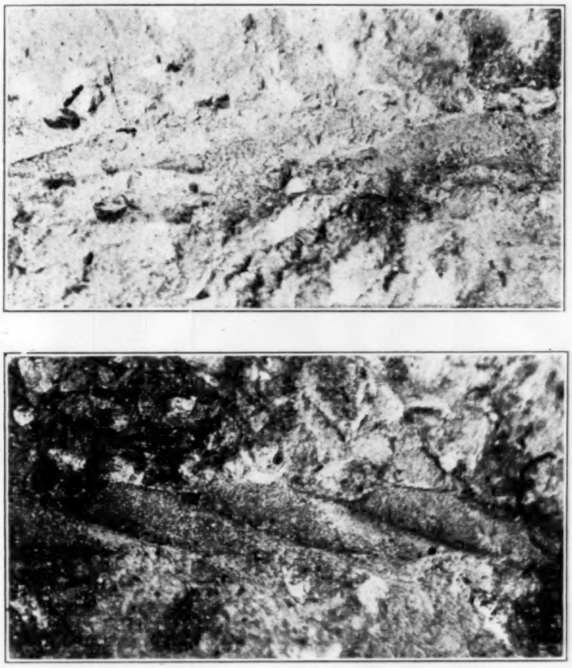


FIG. 36.—Field-Produced Concrete. Texture of Mortar forming Bond on Under Side of Reinforcing Steel; 1 : 2 : 4 Mix; "Sloppy" Consistency.

In reinforced-concrete construction, practically the same conditions exist as are described above for plain concrete, in so far as consistency of mix is concerned. Figs. 32 to 36, inclusive, illustrate these conditions.

In connection with Figs. 32 to 36, inclusive, it must be borne in mind that the consistencies used for the test beams, contained a somewhat greater quantity of water than did those chosen for plain-concrete test cylinders. All concrete used in beam tests was 1 : 2 : 4 mix.

Fig. 32 shows the texture of the mortar forming the bond upon the top or upper side of the reinforcing steel in a concrete of first consistency. The voids, although existing by hundreds, are mere pin-holes. The mortar is gray in color.

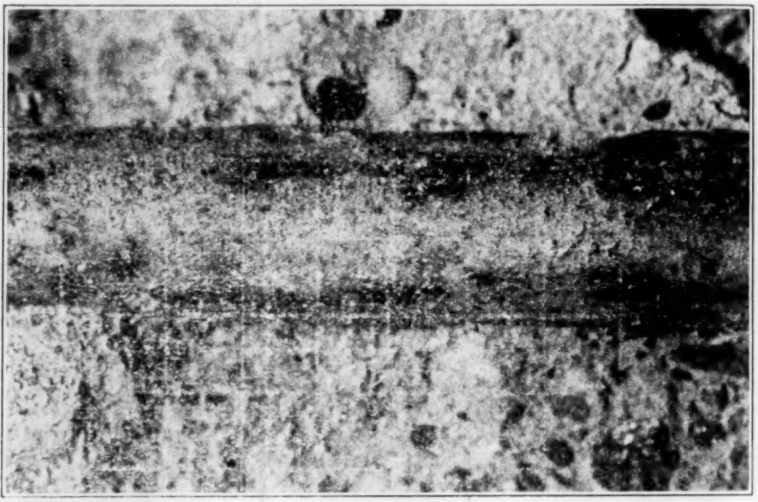


FIG. 37.—Dark Areas indicate Mortar Bond on Reinforcing Steel at Slip. Note Portions of "Laitance" Undisturbed by Slip (× 4).

Fig. 33 shows for concrete of first consistency, the mortar bedding forming the bond on the under side of the reinforcing steel; although it contains hundreds of minute voids it is nevertheless in close contact with the metal. No cavities are visible.

Fig. 34 shows for concrete of fifth consistency, the appearance of the bedding and the character of the bond attained on the upper and lower sides of the reinforcing steel. Fig. 35 shows to better advantage the condition on the under side of the metal.

The water cavities with the barriers of laitance occupy a considerable percentage of the total bonding area.

Fig. 36 shows the results attained in actual practice. The concrete in the portion of the bridge abutment from which this photograph was taken was known to have been of a sloppy consistency when placed.

Fig. 37 is interesting in that it shows the results of failure by the slipping of the reinforcing steel. The dark areas were produced by a breaking down of the laitance walls or ridges, these surfaces taking a polish as if a lubricant had been applied to them. It is of interest to further note that these polished areas exist only along the sides of the under bedding area, and that along the center of the bed the water cavities remain undisturbed, indicating that through the hardening of the concrete the center of the bed may have shrunk from contact with the steel.

In the tests for the strengths of reinforced-concrete beams (see Fig. 15), it was found that those of the fourth and fifth consistencies decreased very materially with the increase of age. It appears reasonable to assume that this decrease is produced by changes in the bonding material, whether by changes in shape or by changes tending to decrease its resistance to crushing is not clear.

In connection with the above, it is interesting to note that one of the earliest American investigators in the field of concrete construction, Brevet Maj. Genl. Q. A. Gillmore,[1] held the opinion concerning Portland cement that "its most prominent and valuable properties are displayed" when mixed with a quantity of water "capable of producing a semi-fluid or creamy consistency."

TEST CONDITIONS *vs.* FIELD CONDITIONS.

The first batch of concrete used in the tests was mixed on March 30, the last batch on July 25. The range of temperature and humidity was therefore comparable with that usually found in northern latitudes in a working season extending from April 1 to October 20. In this connection, however, it must be borne in mind that the actual mixing of the concrete was done between 7.00 and 8.30 a. m. The proportioning, mixing, and placing was doubtless more accurately, uniformly, and carefully done than is usual upon high-grade construction work. The conditions of seasoning were as nearly uniform as could be provided at a nominal expense. All testing of materials, cylinders, beams, etc., was done by experienced operators. It is therefore reasonable to assume that irregularities due to the methods of procedure are comparatively small, and that the results obtained are generally normal.

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[1] "A Practical Treatise on Limes, Hydraulic Cements and Mortars," 1861.

In actual concrete construction operations in the field, the object sought, in so far as the specifications for any particular structure are concerned, is, without exception, to produce a good, reliable material. Results frequently fall short, however, for due to a misleading popular opinion that "anyone can build in concrete," comparatively few field superintendents and foremen have developed a thorough knowledge of concrete materials and of high-class field methods. The hazardous policy of "save the labor and spoil the concrete" is too frequently found in a sort of inseparable combination with the above.

As a result of the conditions already described, it occasionally happens that the materials used are not always of good quality, neither are they always free from admixture with harmful ingredients in dangerous quantities. In the transitory stage the component materials are frequently used in improper proportions; they are insufficiently mixed; the cementing material is not fully developed; and the mass is subjected to more or less abuse, with the eventual result that the concrete produced contains myriads of voids and cavities, and frequently lacks strength, hardness, toughness, and the general durability requisite for good construction material of the class contemplated by modern practice in plain and reinforced-concrete design.

STRENGTH REQUIREMENTS.

As a basis for comparisons assume the ultimate compressive strength of concrete in pounds per square inch, when tested on 6 by 12-in. cylinders, to be as follows:

Age of 1 month, 1 : 2 : 4 mix, 2200 lb. per sq. in.; 1 : 2½ : 5 mix, 1900 lb. per sq. in.

Age of 3 months, 1 : 2 : 4 mix, 2800 lb. per sq. in.; 1 : 2½ : 5 mix, 2300 lb. per sq. in.

Assume also the conditions of mixing, placing, storage, etc., to be the same as existed in the tests already described.

By reference to Figs. 8 and 9, it appears that of the weaker sands Nos. 9, 10, 11 and 12, only sand No. 9 would be acceptable for a 1 : 2 : 4 mix and sands Nos. 9 and 10 for a 1 : 2½ : 5 mix. The stronger sands, Nos. 2, 3, 6 and 7, would give an excess strength of from 25 to 51 per cent for a 1 : 2 : 4 mix, and from 36 to 49 per cent for a 1 : 2½ : 5 mix.

Using relative strengths, as shown in Fig. 13 for a third consistency of mix, all sands used in the tests would fail to fulfil the 1-month age requirements. At the age of 3 months, sands Nos. 2, 3 and 6 would give an excess strength of from 1.5 to 16 per cent for a 1 : 2 : 4 mix; while sands Nos. 2, 3, 6 and 7 would give an excess strength of from 6 to 15 per cent above that required for a 1 : 2½ : 5 mix. Similarly for a fifth consistency of mix, the weaker sands would give approximately 40 to 60 per cent and the stronger sands 65 to 85 per cent of the required strengths.

The results as given under "time of mixing" show that the conditions attending field mixing may be such as to very materially affect the final strength of the concrete produced. The detrimental influences of one or more of the factors investigated in this series of tests frequently exist in connection with actual field construction work. In addition, there are other harmful factors which to a greater or less degree affect the strength and durability of the concrete produced. To the contrary, the generally accepted requirements of modern practice in plain and reinforced-concrete design assumes the existence of good materials, and the use of efficient field methods and operations in their application. Economy of construction requires that the practice of design be upheld. Safety and permanence demand greater knowledge of concrete materials, and greater efficiency in construction methods and operations.

SAND SPECIFICATIONS.

As early as 1855 Mr. T. Hughes, Civil Engineer, in a series of papers[1] described his choice of sand for concrete work as follows:

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[1] "Foundations of Bridges."

"To speak practically of the mixture of sand in making concrete we should say that none of the particles ought to exceed the size of a barley-corn, and that a great portion of the sand should not exceed half that size, while another portion of the sand, equal in quantity to the other two portions united, should consist of grains of sand not so large as a pin's head. The sand to be sharp and clean."

This quotation is interesting, in that it shows the valuable information gained by early artisans and engineers in concrete construction work, to have been ignored by the writers of sand specifications in comparatively recent years. It is only within the past decade that American concrete specifications have provided requirements other than that the sand aggregate shall be sharp and clean.

The author submits specifications for a sand to be used for general concrete purposes, which are as follows:

> Sand shall be of hard, preferably silicious, material, clean, rough, free from dust, soft particles, vegetable loam or other deleterious matter. It shall consist of particles graded from coarse to fine, of sizes that will pass, when dry, a sieve having 4 meshes per linear inch. The grading of particles shall otherwise conform to the following:
>
> Not more than 80 per cent shall pass a sieve having 10 meshes per linear inch, not more than 55 per cent shall pass a sieve having 20 meshes per linear inch, not more than 15 per cent shall pass a sieve having 50 meshes per linear inch, and not more than 5 per cent shall pass a sieve having 100 meshes per linear inch. Upon the 10, 20 and 50-mesh sieves an allowable variation of 5 per cent will be permitted.
>
> Sand, when combined with a normal Portland cement and 1-in. broken granite, limestone or trap of good quality in the proportions 10 lb. of cement, 21 lb. of dry sand, and 35 lb. of dry broken stone, thoroughly mixed with 4¼ lb. of water for not less than 1 minute and molded into cylinders 6 in. in diameter by 12 in. long, shall develop a compressive strength of 1300 lb. per sq. in. when tested at the age of 7 days and a strength of 2200 lb. per sq. in. at the age of 30 days. Strength shall be determined from an average of five cylinders tested at each age. The cylinders shall be removed from the forms 24 hours after molding and shall be stored in a moist closet or in damp sand until tested.
>
> Sand failing to develop the above strengths may, at the option of the Engineer, be accepted for use, provided that the proportion of cement be increased by an amount sufficient to fulfil the strength test requirements.

It is believed that these specifications are adapted to the determination of a satisfactory sand aggregate for general concrete work. Whenever the conditions involved require the use of concrete having special density, toughness, or other physical properties, the fitness of a given sand should be determined by special test.

The specifications assume that provision is made for the proper proportioning of the cement content of the mix, in cases where sands failing to meet the strength requirements are used. It also assumes that proper provision is made for the limiting of the water content of the mix to that required to produce a saturated, sticky, semi-plastic mortar.

It will be noted that the time limits provided for in the specifications are nominally the same as those provided for in the testing of cement.

CONCLUSIONS.

The description of methods used and of results obtained have been given sufficiently in detail to permit the reader to modify and correct the conclusions of the author, accordingly as his greater experience or better judgment dictates. However, from a careful consideration of the results obtained and of the phenomena observed, the following conclusions appear to be warranted:

1. The commonly practiced "visual examination" test of sand aggregate for concrete is generally unreliable, since it gives at best only a superficial knowledge of the cleanliness of a given sand. Its adaptation to the determination of grading could be of value to the observer only after long experience in the granulometric analysis of sands.

2. The generally accepted practice of proportioning a concrete mix by volume, as, for example, 1 part cement, 2 parts sand and 4 parts broken stone, is impracticable and unscientific, since it does not take into account the adaptability of the grading of a given sand to the production of a dense, strong, and reliable concrete. Proportioning by volume, as commonly used, gives no guarantee of the production of a concrete having a desired strength, hardness, or other physical properties.

3. The strength, toughness, and durability of the concrete to be secured from the use of a given sand can be determined

only by an actual test of that sand in a properly prepared concrete.

4. In field operations incident to spading, slicing, or otherwise compacting the concrete, the movement of the water content of the mass is intensified, whenever the sand aggregate contains insufficient fine material to hold the cement in suspension by the formation of an adequate amount of sandy paste. The free movement of the water tends to produce an improper distribution of the cement.

5. The use of a quantity of water sufficient to produce a concrete, the mortar component of which is of a saturated, sticky, semi-plastic consistency, is for most practical purposes required, in order to facilitate economical and efficient placing. This quantity of water is ample for the development of the proper functions of the cement. An increase in the quantity of water used results in a proportionate decrease in the strength of the concrete. This decrease is in no sense a function of the proportions of the mix.

6. The excess water in an over-saturated concrete necessarily occupies space and thereby bulks-up the mass. By reason of its high surface tension, it forms water globules which, although somewhat affected by the weight of the concrete, are nevertheless distributed throughout the mortar component and are accumulated underneath the particles of the sand and stone aggregates and the reinforcing steel. By evaporation, this excess water ultimately disappears, leaving a considerable volume of water voids and cavities which constitute an extremely important factor in the strength and reliability of the concrete.

7. The critical failure of reinforced concrete depends upon the intensity of the bond existing between the concrete and the steel reinforcement. Concrete containing an excess of water not only develops less surface contact with the steel on account of the resulting increase in the volume of water voids and cavities; but, in addition, the excessive laitance produced by the water tends to accumulate around the reinforcement, thus contributing materially to a decrease in strength. This condition becomes further aggravated by reason of the tendency of the laitance to become less resistant with age.

8. For the various grades of concrete, the minimum ulti-

mate strengths assumed in the modern practice of plain and reinforced-concrete design are not assured by the commonly specified requirements for sand and stone aggregates, and by the present lack of uniformity and of efficiency in field methods and operations.

9. The results obtained show no definite relation between the compressive strengths of 1 : 3 mortar cubes, and the compressive strengths of the concretes produced from the same sands.

Acknowledgments.—The author takes sincere pleasure in here giving due credit to those who have rendered worthy assistance in various portions of the work, in the making of test specimens, chemical and physical tests, and tabulation of data.

Special mention is due Messrs. C. Dennis, A. S. Goss, R. G. Goss, W. O. Hutchins, F. G. Marriott, R. J. Marshall, C. J. Townsend and M. Walsh.

DISCUSSION.

Mr. Wig. MR. R. J. WIG.—This is an excellent paper. It contains data confirming tests of a similar nature made in the Bureau of Standards Laboratories. There is one curve shown in the paper (Fig. 13) illustrating the modulus of elasticity of 8,000,000 lb. per sq. in. for 1 : 2 : 4 concrete. This is extremely high; in fact I do not recall ever obtaining such a modulus for 1 : 2 : 4 concrete, or in fact for richer mixtures.

Mr. Edwards. MR. L. N. EDWARDS.—The strengths obtained in the greater part of the tests are, I believe, above the average, possibly due in part to the use of sands of limestone origin. The high modulus of elasticity for first consistency cylinders was no doubt due to the great strength of the cylinders tested. The slight curvature of the stress-strain curves for first consistency concrete shown in Fig. 13 indicates the rigidity of these cylinders. With one exception, compressometer tests were made upon two cylinders of each mix and consistency.

Mr. Wig. MR. WIG.—Many cylinders subjected to a stress of over 6000 lb. per sq. in. in our laboratories have had much lower moduli than shown by this paper. The results given appear extremely high.

Mr. Abrams. MR. D. A. ABRAMS.—This question of the effect of the size and grading of the aggregate and its influence on the strength of concrete and mortars is a very important one, and one which has received a great deal of study during the past year or two. Some points are brought out by Mr. Edwards which have an important bearing on this subject, but I shall dwell on only one of them at this time. It strikes me that the specification for sand which has been proposed is subject to the same criticism that most of our present specifications for sand are open to, namely, that the attempt is made to formulate a specification by placing limitations on certain sizes of material or the percentages which fall between certain sieve sizes. That kind of specification has been in use, but as a proper basis for specifications for concrete aggregates it is absolutely fallacious.

Mr. Abrams.

Our own tests have shown this, and many other sands can be found and any number of them can be made up synthetically which will violate all of the proposed requirements, and not only violate them but violate them grossly, and yet give just as good results as other sands that fall within the limits fixed. For instance, we have made concrete tests using a sand, 40 per cent of which passes the 100-mesh sieve. That is very greatly beyond any permissible limits according to our specifications, yet, due to the fact that the remaining aggregate in the mix was suitably graded, this kind of sand gave just as good concrete as the others which conform to the usual requirements. Therefore, it is not a question of the size of material falling between certain limits, but the relative distribution of sizes which must be taken into account in arriving at an estimate of the suitability of a sand for concrete. In other words, no one portion of the concrete aggregate can be required to conform to limitations, because the results secured will depend altogether upon the relative sizes of the remaining particles.

With reference to the question of fine sand, it is not my intention to suggest that fine sand should be deliberately selected; but the fact that fine sand can be used, if it is properly proportioned, should be taken into account in formulating such specifications.

Mr. Johnson.

MR. NATHAN C. JOHNSON (*by letter*).—Mr. Edwards' paper is deserving of careful consideration both for its direct results, and more particularly for its re-revealing of the present condition of the concrete art. Concrete has indeed "become a structural material of first importance;" it has too truly "been studied as a single material" in which base components are transmuted by the alchemic touch of Portland cement into some strange unknown endowed with wondrous abilities, unsubject to limitation or law; and "widely varying results, affecting the strength and permanence of structures built wholly or in part of concrete" have been and are so generally and unexplainably produced that the industry as a whole is at last becoming aroused to the imperative necessity of finding means which will give real assurance as to the integrity of future constructions. This, perhaps, is the most fortuitous result thus far accomplished by all recent investigations. Slowly but surely we are all becom-

Mr. Johnson. ing radicals; and tempered radicalism is the well-spring of progress.

The scope and conduct of the tests given in this paper are progressive and excellent. It is trite to say that they are not essentially new, or that they run parallel to many thousands of tests made elsewhere. This parallelism is of no consequence, for information, not heretofore fully obtained so far as the writer is aware, is made available through correlation of visual evidence with that obtained from physical strength tests.

It is without intent to disparage or to declare anticipation, but rather by way of appreciation, that the writer refers to his own photographic studies of concretes in connection with those by Mr. Edwards. In particular, as relating to the strength of concrete, the internal arrangement with different mixtures, and the occlusion of air at aggregate and steel surfaces, with faulty adhesion of cement, are of interest and importance. In the writer's files are some hundreds of aggregate surfaces such as those shown by Mr. Edwards, with like isolation found almost universally, in good concretes as well as bad; and in discussing the reason for the low strength of concrete made from high-strength materials, the importance of these air and water voids has been pointed out.[1]

The value of this paper, however, is not so much in its absolute results, as in confronting all of us anew with the necessity for modified procedures and measures to remedy present defects. The sneering reference to "millenial super-concretes" so often met with, or the blunt denial of countenance for improved procedures from the man who "has used concrete for twenty-six years and knows all he needs to," cannot and does not alter the necessity for some way out. Yet until this way is found and demonstrated there should be every sympathy for those to whom concrete is the "good old mud that stays put" at so much per yard and by whom the idea of improvement is either regarded as an unwarranted interference that may cost money, as an idle dream that once in a while bedevils the easy rules of the game, or else with a sense of wonderment that any one should want to improve a material that always has been so obviously unbitable.

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[1] *Engineering Record*, January 23, 1915; "The Microstructure of Concrete," *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 171 (1915).

Mr. Johnson.

The demand for improvement is of comparatively recent origin. Cement previously bore all burdens. It still bears many, but they are lessening as knowledge grows. Contamination of aggregates; classes and kinds of aggregates; grading of aggregates; mixing; proportioning; and more recently, quantity of water, have been forced to assume a portion of the load.

But field results do not materially improve. Engineering knowledge is bulky about concrete and how to make it, but *what use is made of it?* Beyond perfunctory inspection of sand or stone, mill inspection of cement and tests of reinforcing steel, concrete making and placing is to-day—as it was in the beginning—of mediaeval simplicity. Specifications are solemnly written and agreed to in contracts, but the final arbiter is, in essence, a man who never heard of either, whose "twenty-six years' experience" have hardened him in wrong-doing and whose task-master exacts of him a certain toll per diem; and, like the Israelites, he escapes punishment only by showing his handiwork at sundown, with or without straw, regardless of value or endurance.

Is it not true that engineering opinion is largely untutored in regard to concrete? Do not "usual procedures" govern in the execution of a contract, regardless of the letter of law in a specification? Does not a contractor, and the average engineer as well, consider actual, specific performance an unwarranted hardship? It is the belief of many that not until specifications are so written as to embody our best knowledge and are accepted and enforced in their literal sense, can general improvement be expected.

As an instance of automatic nullification of specifications, consider the vital question of proportioning. Specifications, written perhaps by one who knows better, stipulate cement, sand and stone in arbitrary proportions of 1 : 2 : 4, regardless of what aggregate is used; and with water (inferentially) *ad libitum.* On the job, wheelbarrows, loaded without any care, or an unstruck, uncalibrated hopper, possibly brought from another job where some other ratios were in order, with sand wet or dry, and stone from several sources, both sand and stone varying in grading, are used week after week in the fabrication of an important structure, with no comment excited.

Mr. Johnson. This is but one example of one step out of many, any or all of which profoundly affect the resulting concrete. Everyone, or nearly everyone, knows that proportions should be determined for each combination of given materials and adherence to proportions so determined made both obligatory and possible by some means, such as by automatic weighing. The quantity of water should be rigidly regulated. Better mixing by improved machines without entraining of air as at present is not only a necessity but is well within reach. The benefits of temperature control in the usage of cement should be taken advantage of. And last, but not least, placing of concrete should be such as not to negative the care previously taken in its compounding. The present usage of concrete is almost primitive, but papers like that of Mr. Edwards are indicators of progress. If we have not previously known we were in a slough, we realize it now; and this realization is guaranty that we shall draw out of it.

Mr. Edwards. MR. EDWARDS (*Author's closure by letter*).—The practical value of any series of tests, whether made upon concrete or other materials of construction, is in direct proportion to the application of such tests to the raw materials used, the construction methods and operations involved, and the final results which may be obtained.

Without entering into a detailed discussion of Mr. Johnson's comments, it seems desirable to add emphasis to the author's statement, namely: "In a well-prepared and properly placed concrete the actual volume of the voids and cavities produced by occluded air is relatively small as compared with that produced by the water content of the mix." Due to its lighter weight, air entrained in concrete during mixing and placing frees itself quite readily whenever "a seasonable amount of work is done in connection with the compacting of the concrete during placing." Water, by reason of its greater weight, is more readily held in suspension within the mortar content of the concrete, where it ultimately forms water voids and cavities. While "better mixing by improved machines without entraining of air," to quote Mr. Johnson, would no doubt result in a general improvement of concrete, to be consistent in their use the unloading from such a mixer and the trans-

Mr. Edwards.

portation and final placing of such concrete must be done without the intermixing of air. Upon very many classes of structures this would involve the use of a complicated, cumbersome, and rather costly outfit, which, from practical and economic points of view entirely outweighs the comparatively small advantages to be attained by the use of concrete containing no air voids.

EFFECT OF RATE OF APPLICATION OF LOAD ON THE COMPRESSIVE STRENGTH OF CONCRETE.

BY D. A. ABRAMS.

SUMMARY.

In attempting to formulate a standard method for making compression tests of concrete, it is essential that the influence of differences in the rate of application of load be fully understood.

Tests were made at the Structural Materials Research Laboratory, Lewis Institute, Chicago, on 270 concrete cylinders 6 in. in diameter by 12 in. long, mixes 1 : 9, 1 : 5 and 1 : 3. The aggregate consisted of sand and pebbles graded in size to 1½ in. Each cylinder was made from a separate batch, mixed by hand. Tests were made at the age of 28 days.

Two methods of loading were used, which were as follows:

Method No. 1.—About 10 per cent maximum load was applied at a fast speed (0.25 in. per minute); remainder of load applied at rates varying from 0.006 to 0.15 in. per minute.

Method No. 2.—The test specimens were loaded to 3 to 88 per cent of their maximum load at a fast speed (0.25 in. per minute); the remainder of the load was applied at about 0.01 in per minute.

In compression tests of concrete the rate of application of load has an important influence on the strength. For the richer mixes (1 : 5 and 1 : 3) tested at the age of 28 days, the strength when loaded at 0.15 in. per minute is 14 to 20 per cent higher than when loaded at 0.006 in. per minute.

If part of the load is applied at a fast speed and the remainder at a slow speed, the ultimate strength of the concrete is not changed, even though as much as 88 per cent of the total load be applied at the fast speed.

Knowing the approximate strength of the specimen, it will greatly expedite the work of testing, and involve no sacrifice in accuracy, if we apply, say, 50 to 75 per cent of the ultimate load at a fast speed before changing to the slow speed.

A machine speed which gives a shortening of the test piece of 0.01 to 0.02 in. per minute per foot of length is recommended as a standard rate for compression tests of concrete.

The results show that the mean error of a group of tests made on the same day is considerably lower than that of a set of tests made on different days; while the mean error of the daily average compressive strengths is generally much higher than that of the averages of tests made on different days.

EFFECT OF RATE OF APPLICATION OF LOAD ON THE COMPRESSIVE STRENGTH OF CONCRETE.

By D. A. Abrams.

Experience in testing structural materials has shown that the ultimate strength and other properties of the material are affected by the rate of application of load. The importance of this subject is recognized in certain forms of testing. For instance, in tests of metal certain limits in the rate of application of load are recommended. A definite rate of loading forms an essential feature of the specifications for tension tests of cement briquettes.

In order to arrive at standard methods for making compression tests of concrete, it is essential that the influence of different rates of loading be fully understood. The tests described were carried out for the purpose of securing information on this subject.

Compression tests were made on 270 concrete cylinders, 6 in. in diameter by 12 in. long, mixes 1 : 9, 1 : 5 and 1 : 3, the aggregate being graded up to $1\frac{1}{2}$ in. All tests were made at the age of 28 days by two methods, which are as follows:

Method No. 1.—About 10 per cent of the total load was applied at a fast speed (0.25 in. per minute); the remainder of load at rates varying from 0.006 to 0.15 in. per minute.

Method No. 2.—The cylinders were loaded to 3 to 88 per cent of their maximum load at a fast speed (about 0.25 in. per minute); the remainder of the load was applied at about 0.01 in. per minute.

These tests were made as a part of the experimental work on concrete and concrete materials being carried out by the Structural Materials Research Laboratory, Lewis Institute, Chicago.

Materials and Test Specimens.

The concrete was made from Portland cement, sand and pebbles. The aggregate was obtained from the Hammond, Ill., pit of the Chicago Gravel Co.

Prior to use a quantity of sand sufficient for this series was thoroughly mixed and placed aside. The pebbles were screened into three sizes and recombined for each test specimen. Tests on samples of the aggregate as weighed out for use on six different days gave the average sieve analysis shown in Table I.

The mixes were proportioned by volume, using one volume of cement to 3, 5 or 9 volumes of mixed aggregate. The material for each cylinder was proportioned and mixed separately. The concrete was mixed by hand by means of a bricklayer's trowel, using shallow flat-bottomed galvanized iron pans about 24 by 27 in.

The consistency was such that only a small quantity of free water was brought to the surface by puddling as hereinafter described. The amount of water used was 6.2, 7.0 and 8.0 per cent of the weight of dry materials in the 1 : 9, 1 : 5 and 1 : 3 mixes, respectively.

TABLE I.—AVERAGE SIEVE ANALYSIS OF AGGREGATE.

Sieve No.	Size of Square Opening, in.	Amount Finer than Each Sieve, per cent by weight.
100	0.0058	1
48	0.0116	3
28	0.023	14
14	0.046	23
8	0.092	33
4	0.185	41
$\frac{3}{8}$ in.	0.37	51
$\frac{3}{4}$ in.	0.75	73
$1\frac{1}{2}$ in.	1.5	100

The test specimens were molded by placing the concrete in metal forms in layers of 3 to 4 in. in thickness, puddling with a $\frac{3}{4}$-in. steel bar about 2 ft. long. The forms were provided with machined cast-iron base plates. After puddling the last layer the concrete was leveled off with the trowel. In order that the specimens might present plane surfaces to the bearing plates of the testing machine, the tops were capped by means of a layer of neat cement paste about 2 to 4 hours after the concrete was placed. The cap was made smooth by pressing down a piece of plate glass which was left in place until the form was removed.

The specimens were left in the forms one day and then removed and stored in damp sand until 28 days old. Each set of specimens consisted of 6 cylinders, which were made on 6 different days.

METHOD OF LOADING.

A 200,000-lb. Olsen testing machine was used. The two methods of applying the load used in the tests have been previously described. The rate of loading was determined by direct measurements of the shortening of the cylinder during the test. In the machine used, the rate of application of load varies somewhat with the amount of load; consequently, the indicated rates are the averages for the six tests in a set. It should be noted that the rate of loading is not the same as the rate of movement of the machine head when running idle; the idle speed may be 2 to 4 times as fast as the speed under load, due to the deflection of the levers, elongation of screws, compression of cushions, etc.

DISCUSSION OF TESTS.

Table II gives the average values from the tests by method No. 1, in which all cylinders were loaded to about 10 per cent of their maximum, before applying the remainder of the load at rates varying from 0.006 to 0.15 in. per minute. The curves shown in Fig. 1 indicate that the strength of all mixes is appreciably affected by varying the rate at which the load is applied throughout the test. The relative effect seems to be a function of the strength and modulus of elasticity of the concrete. When shortened at the rate of 0.15 in. per minute the cylinders showed a compressive strength 6, 14 and 20 per cent higher than when loaded at 0.006 in. per minute, for the 1 : 9, 1 : 5 and 1 : 3 mixes, respectively.

The tests made by method No. 2 (see Table III and Fig. 2) show that the compressive strength of concrete is not affected by applying as much as 88 per cent of the maximum load at a fast speed, providing the remainder of the load is applied at a slow speed; in other words, it is the rate of application of only the last 12 per cent (or less) of the load that affects the compressive strength of the concrete.

The relative effect of different rates of application of load appears to depend on the modulus of elasticity and the compressive strength of the concrete. For concrete made and stored in the same way there is a nearly constant relation between the modulus of elasticity and the compressive strength; consequently the results of these tests may be considered of general application within the range of loads used, for specimens stored in a damp place. For specimens stored under different conditions the results may not apply directly.

TABLE II.—EFFECT ON RATE OF APPLICATION OF LOAD ON THE COMPRESSIVE STRENGTH OF CONCRETE.

METHOD NO. 1.

Load of about 10-per-cent maximum was applied at a fast speed (0.25 in. per minute); remainder of load applied at rates shown.

Specimens were 6 by 12-in. cylinders.

Age at test 28 days; stored in damp sand, tested damp.

Rates of movement of the machine head were determined by direct measurement of the shortening of the cylinder during loading. In this machine the rate of motion of the head for a given speed varies somewhat with the amount of load, so that the speeds given are the averages for tests of six specimens in a set.

Each value is the average of 6 tests. The cylinders in a set were made at intervals of 1 to 4 days.

Reference No.	Rate of Movement of Machine Head, in. per min.	Compressive Strength, lb. per sq. in.		
		1 : 9 Mix.	1 : 5 Mix.	1 : 3 Mix.
1	0.006	800	1740	2840
2	0.010[a]	800	1790	2910
3	0.015	820	1870	3140
4	0.035	800	1950	3210
5	0.040	850	1920	3260
6	0.060	840	1980	3320
7	0.150[b]	850	1970	3400

[a] Usual rate of loading for compression tests of concrete made in this Laboratory. The rate of movement of the head when running idle is considerably higher than the values given; for the lower rates the idle speed is 2 to 4 times the shortening of a 12-in. cylinder under load.

[b] Fastest speed at which a proper determination of the maximum load could be obtained.

These results have a direct bearing on standard compression tests of concrete, since such tests are generally made on concrete stored in a damp place. In general, the rate of loading which can be used satisfactorily will fall between 0.01 and 0.02 in. per minute. This, of course, refers to the actual rate of shortening of a 12-in. test specimen. A rate below 0.01 in. per minute unnecessarily prolongs the time required for testing; a rate faster than 0.02 in. per minute offers no advantages, especially since there is shown to be no objection to loading the specimen well up to the maximum at a faster speed. The richer mixes (1 : 5 and 1 : 3) show about 7 per cent higher strength when loaded at 0.02 in. as compared with 0.01 in. per minute.

On account of variations in the action of testing machines of different capacities and designs, it is impracticable to adhere strictly to a certain predetermined rate of loading. It is suggested that the specimens be loaded from 50 to 75 per cent of maximum at a fast speed, and that the remainder of the load be applied at speeds which will give a shortening of the specimen of 0.01 to 0.02 in. per foot of length per minute.

TABLE III.—EFFECT OF RATE OF APPLICATION OF LOAD ON THE COMPRESSIVE STRENGTH OF CONCRETE.

METHOD NO. 2.

Three to 88 per cent of the total load was applied at fast speed (0.25 in. per minute.) The remainder of the load was applied at about 0.01 in. per minute.

Specimens were 6 by 12-in. cylinders.

Age at test 28 days; stored in damp sand, tested damp.

Each value is the average of six tests made on six different days.

See Table IV for details of tests.

Reference No.	1 : 9 Mix.			1 : 5 Mix.			1 : 3 Mix.		
	Percentage of Total Load Applied at Fast Speed.	Compressive Strength, lb. per sq. in.	Mean Error, per cent.	Percentage of Total Load Applied at Fast Speed.	Compressive Strength, lb. per sq. in.	Mean Error, per cent.	Percentage of Total Load Applied at Fast Speed.	Compressive Strength, lb. per sq. in.	Mean Error, per cent.
1.....	10	770	11.2	5	1800	3.4	3	3120	7.5
2.....	12	780	8.5	11	1820	6.1	12	3050	8.7
3.....	25	770	7.5	22	1850	2.9	21	3040	5.4
4.....	45	760	11.2	42	1860	4.8	42	2970	3.9
5.....	59	760	5.5	60	1870	5.5	61	3040	3.9
6.....	70	780	9.9	68	1860	5.9	68	3070	5.4
7.....	79	800	7.5	77	1850	3.1	79	3000	5.0
8.....	86	780	8.0	88	1800	8.9	88	3020	4.0
Grand Av.	....	776	8.7	....	1841	5.1	....	3038	5.5

MEAN ERROR OF TESTS.

The tests made by method No. 2 gave results which are practically independent of variations in loading. Therefore, these values have been used as a study of the mean errors of tests made on the same day as compared with similar tests made on different days. The details of the tests made in this way are given in Table IV, which shows results of tests of specimens made on the same day, together with the corresponding averages and mean errors. Likewise, the averages and mean errors are given for specimens made and tested on different days.

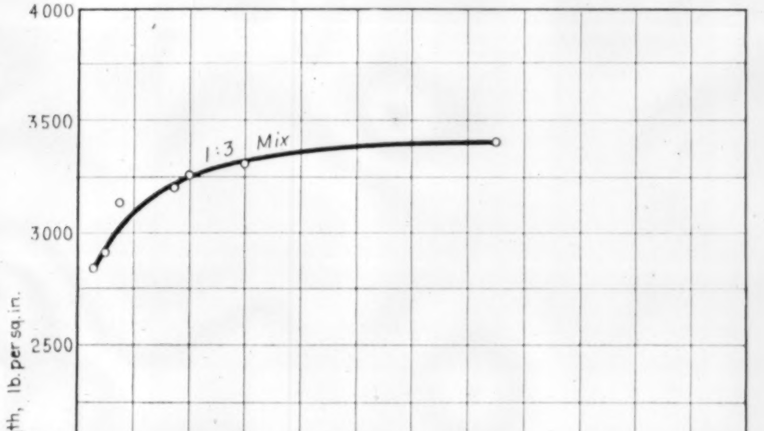

FIG. 1.—Effect of Rate of Loading on Compressive Strength of Concrete. Method No. 1 (see Table II for details).

All mean errors were computed by the formula:

$$\sqrt{\frac{\Sigma v^2}{n-1}}$$

where v is the difference between the individual value and the average, and n is the number of tests considered. The mean errors of the averages were referred to the grand averages.

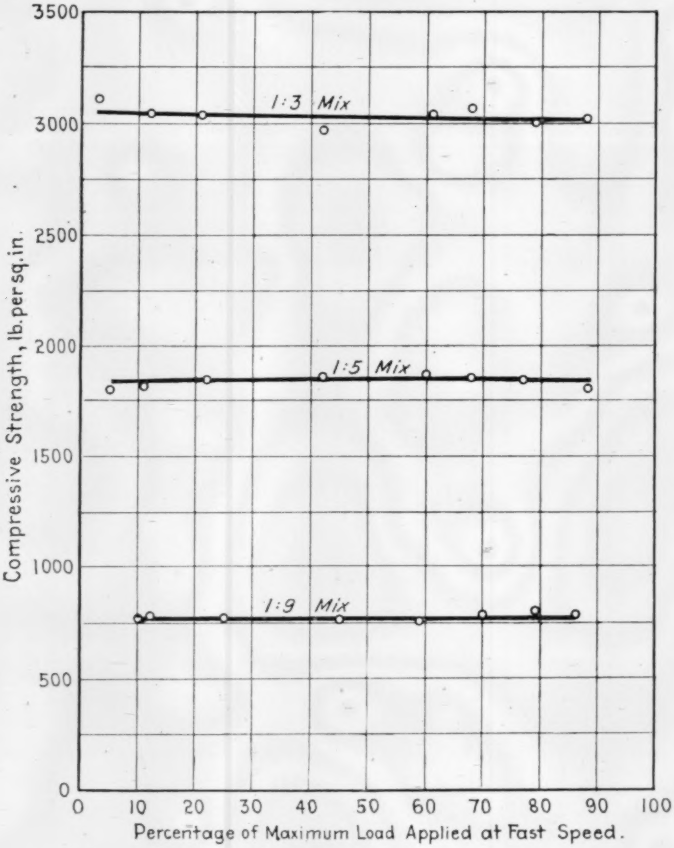

FIG. 2.—Effect of Rate of Loading on Compressive Strength of Concrete. Method No. 2 (see Tables III and IV for details).

Attention is here recalled to the fact that each specimen was made from a single batch of concrete which was proportioned and mixed separately. This method of making the speci-

TABLE IV.—A STUDY OF MEAN ERRORS OF TESTS MADE BY METHOD NO. 2.

Three to 88 per cent of the total load was applied at a fast speed (0.25 in. per minute); the remainder of the load was applied at about 0.01 in. per minute.

See Table III for percentages of total load which were applied at fast speed.

On account of the uniformity of the test results the variations in the methods of application of load are disregarded here.

Reference No.	Compressive Strength, lb. per sq. in.							Mean Error, per cent.
	First Day.	Second Day.	Third Day.	Fourth Day.	Fifth Day.	Sixth Day.	Average.	
1 : 9 MIX.								
1	930	730	800	780	720	690	770	11.2
2	890	720	810	780	780	710	780	8.5
3	830	770	840	760	730	690	770	7.5
4	900	710	770	810	670	700	760	11.2
5	810	730	790	790	730	710	760	5.5
6	920	750	780	790	750	690	780	9.9
7	850	720	860	820	790	730	800	7.5
8	830	700	880	750	810	730	780	8.0
Average	870	730	820	790	750	710	776[a]	8.7
Mean error, per cent	5.3	3.1	4.9	3.0	6.0	2.4	4.1	8.6[b]
1 : 5 MIX.								
1	1840	1790	1870	1840	1700	1770	1800	3.4
2	1790	1910	1950	1830	1840	1630	1820	6.1
3	1870	1910	1880	1880	1790	1780	1850	2.9
4	1910	1860	1980	1870	1850	1710	1860	4.8
5	1920	1840	1930	2020	1780	1750	1870	5.5
6	1920	2020	1890	1840	1750	1730	1860	5.9
7	1910	1880	1890	1880	1810	1760	1850	3.1
8	1990	1880	1950	1690	1640	1640	1800	8.9
Average	1890	1890	1920	1860	1770	1720	1841[a]	5.1
Mean error, per cent	3.2	3.5	2.1	4.9	3.9	3.4	3.5	4.2[b]
1 : 3 MIX.								
1	3370	3310	2890	2790	3160	3220	3120	7.5
2	3190	3380	2630	2890	3180	3020	3050	8.7
3	3190	3180	2760	3020	3120	2960	3040	5.4
4	3120	3020	2910	2820	3070	2890	2970	3.9
5	3160	3100	2830	2980	3110	3050	3040	3.9
6	3340	3150	2930	3000	2890	3110	3070	5.4
7	3180	2880	3060	2760	3030	3070	3000	5.0
8	2960	3160	3070	3010	2840	3070	3020	4.0
Average	3190	3150	2890	2910	3050	3050	3038[a]	5.5
Mean error, per cent	4.0	5.0	5.1	3.7	4.1	3.2	4.2	4.0[b]

[a] Grand average compressive strength.

[b] Mean error (per cent) of the daily averages.

mens is very desirable in studies of this kind, since all accidental variations are permitted to exert their full effect. The effect of accidental variations is largely eliminated by making six specimens of a kind.

It will be seen that there is no systematic relation between the mean errors of the tests and the order of making. In other words, the early specimens in a set show no greater variations than the later ones; nor do the specimens made on the first day show any greater variation than those made on other days.

It will be noted, however, that the mean errors of the eight tests made on the same day are considerably lower than that of the six tests made on different days; the same statement may be made with reference to any six tests made on the same day, although the values for the latter conditions are not given. While this is true, Table IV shows that the mean errors of the average strengths of specimens made on different days are much smaller than for the average strengths of specimens made on the same day.

CONCLUSIONS.

In compression tests of concrete, the rate of application of load has an important influence on the strength. For the richer mixes (1 : 5 and 1 : 3) tested at the age of 28 days, the strength when loaded at 0.15 in. per minute is 14 to 20 per cent higher than when loaded at 0.006 in. per minute.

If part of the load is applied at a fast speed and the remainder at a slow speed, the ultimate strength of the concrete is not changed, even though as much as 88 per cent of the total load be applied at the fast speed.

Knowing the approximate strength of the specimen, it will greatly expedite the work of testing, and involve no sacrifice in accuracy, if we apply, say, 50 to 75 per cent of the ultimate load at a fast speed before changing to the slow speed.

A machine speed which gives a shortening of the test piece of 0.01 to 0.02 in. per minute per foot of length is recommended as a standard rate for compression tests of concrete.

The results show that the mean error of a group of tests made on the same day is considerably lower than that of a set of tests made on different days; while the mean error of the daily average compressive strengths is generally much higher than that of the averages of tests made on different days.

DISCUSSION.

Mr. Enright.

MR. BERNARD ENRIGHT (*presented in written form and read by the Secretary*).—The very excellent paper of Mr. Abrams draws attention to its bearing "in attempting to formulate a standard method for making compression tests," and while the reference is principally or wholly to concrete, presumably its bearing would be largely as pronounced in connection with standard mortars. This would seem to be particularly true in view of the fact that a large proportion of these tests are on mixtures of one part cement to three parts of aggregate (the standard compression mortars being one part cement to three parts standard sand) and despite the fact that the proportions are by volume, in the first case, and by weight, in the latter. Unlike the standard sand, the aggregate used might vary considerably in size, but, as Mr. Abrams says in reference to these results, "all accidental variations are permitted to exert their full effect," and the variations apparently are not great.

On the 1 : 3 mix, at the end of 28 days, the compressive strengths obtained by Mr. Abrams, as shown in Table II, range from 2840 to 3400 lb. per sq. in. It may be of interest to note, in this connection, that on 1 : 3 mix, after 28 days air and water hardening, the German Association Laboratory reports, on all the cement manufactured in Germany during the periods shown, an average compressive strength of 4984 lb. per sq. in. for 1910, 5112 lb. per sq. in. for 1911, 5368 lb. per sq. in. for 1912 and 5353 lb. per sq. in. for 1913. Their "28-day exposure to water tests" are lower than these but still far in excess of the figures obtained by Mr. Abrams.

A comparison of these results might lead to the conclusion that German cements are not only superior, but decidedly superior, to the American products. Experiments and tests made by the writer, over a great many years, to develop the facts in just such comparisons, however, show absolutely that this is not the case. It would hardly be pertinent to go into detail here on the matter, but it might be added that the subject

Mr. Enright. is fully covered in my discussions last year,[1] and also at this session, in connection with the work of Committee C-1 on Cement.[2]

Mr. Lesley. MR. ROBERT W. LESLEY.—I think a comparison of many of the strength tests obtained in German laboratories with those obtained in this country are probably misleading, due to the fact that the German test pieces are machine-made under high pressure.

Mr. Abrams. MR. D. A. ABRAMS.—It seems to me that the criticism in Mr. Enright's discussion is entirely foreign to the point. It is scarcely proper to compare standard sand tests with concrete. Provided that other conditions are the same, we might have used any other mixture or any other material and it would have made no difference on the points brought out in this paper; consequently the actual loads recorded there do not have any bearing. It was not our intention to make any comparison of the strengths of concrete nor the properties of cement; we were here concerned solely with the determination of the effect of rate of application of load, using concretes which varied in strength over a wide range.

Mr. Kinney. MR. W. M. KINNEY.—The discussion presented by Mr. Enright would seem to cast a reflection on the intelligence of the engineers of other countries. Certainly the engineers of foreign countries are not going to compare results obtained on one size and shape of specimen with one type of aggregate, with results obtained on another size and shape of specimen with other aggregates. There is not going to be any loss in credit to American cement because lower results are obtained on different shaped specimens. These results were obtained on cylinders, and the German standard is a cube which we know gives considerably higher results.

Mr. Edwards. MR. L. N. EDWARDS.—In the latter part of March, Mr. Abrams told me of the experiments which are described in his paper. Since that time I have tested about 100 cylinders, 6 in. in diameter by 12 in. long. As these cylinders were in series of five, in some cases three were tested at slow speed throughout and two at a higher speed up to about 60 to 70 per cent of their ultimate strength, completing the test at a slow speed, while in other cases this order was reversed. The cylinders were 1 : 2 : 4 and 1 : 2½ : 5 mix and one year old. I am satisfied that the two methods of testing give equally satisfactory results. The time saved by the combined speed method is considerable. **Mr. Edwards.**

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part I, p. 214 (1916).

[2] *Ibid*, Vol. XVII, Part I, p. 198 (1917).

Mr. H. J. Force.—I should like to add just a word to what has been said. During the past few months we have tested several hundred cylinders 8 by 16 in., at ages of 2½ to 3½ years. We found that there was very little difference in the strength obtained at testing speeds of 0.2 in. or 0.1 in. per minute. These cylinders were 1 : 2 : 4 and some 1 : 2½ : 5 "mixer" concrete. Different mixtures might show different results, of course, depending upon the age of the cylinders. **Mr. Force.**

Mr. C. G. Brown.—More for a point of information than anything else, I wish to compare Figs. 1 and 2. I assume that they refer to the same mixes, and that the same concrete was used in each case. For the 1 : 9 mix, it would seem that the results in Fig. 2 are lower than anything shown for this mix in Fig. 1, but they seem to check up better with the values in Fig. 1 for slow speed of testing. Similarly, the 1 : 5 mix in Fig. 2 checks up fairly well with the same mix in Fig. 1 for a moderately slow speed. On the other hand, the 1 : 3 mix, if it checks up at all, checks up with a somewhat faster rate of speed. **Mr. Brown.**

Mr. Abrams.—The speaker has called attention to the very point that is brought out by the tests. In the first series of tests, the first part of the load was applied at a fast speed, but the last part was applied at the very slow speed which he mentions. These tests were made at essentially the same time, but they do not cover exactly the same period, so that accounts for the slight variations in the results of the two sets of values. In other words, one series of tests was started a little ahead of the other; the second series was somewhat of an after-thought. I believe that the symmetry of the curve produced by the points on these diagrams is very seldom found in concrete experiments; in other words, the relative values are unusually consistent among themselves. **Mr. Abrams.**

TESTS OF CONCRETE SLABS TO DETERMINE THE EFFECT OF REMOVING EXCESS WATER USED IN MIXING.

By A. N. Johnson.

SUMMARY.

The object of conducting the series of tests presented in this paper was to measure the effect, if any, that the different methods of finishing a concrete surface, as applied to road work, have upon the strength of the concrete slab as finally laid. The tests were made upon slabs 5 ft. long by 2½ ft. wide and 5 in. thick, some of which were rolled with a light roller and others finished in the usual manner with a hand wood float. Some of the slabs were mixed with a small amount of water to give a medium consistency, while others were mixed with a larger amount to give a consistency more nearly that used in ordinary practice.

The results show that the slabs finished with the roller averaged 20 per cent increase in transverse strength.

These tests demonstrate the beneficial effect of proper manipulation of concrete after it is laid.

TESTS OF CONCRETE SLABS TO DETERMINE THE EFFECT OF REMOVING EXCESS WATER USED IN MIXING.

By A. N. Johnson.

The importance of proper water content for concrete is generally recognized. The amount of water producing a maximum strength makes such a stiff mixture that it is impracticable in most concrete road work. It is necessary, therefore, to use a certain excess of water above that which will give the greatest strength.

In concrete road work, owing to the method of handling, it is of considerable convenience to use a fairly wet mixture. It has been found, however, that much of the excess water can be removed by proper manipulation of the concrete after it is placed. As a consequence some methods of finishing concrete roads have been productive of exceptionally good results. A study of the methods employed shows that while they differ as to detail, in each case the concrete surface has been so treated as to remove a considerable amount of the excess water, thereby making the surface more compact and dense.

Of the different methods which have been observed, that devised by Captain J. J. Gaillard, City Engineer of Macon, Georgia, seems to be the most efficient and least expensive. This method consists in rolling the surface of the concrete with a light roller about 8 in. in diameter and 5 to 6 ft. in length, attached to the end of a long pole. The roller is made of light sheet metal; the total weight of a 6-ft. roller being in the neighborhood of 70 lb. The operator stands at one side of the roadway and rolls the newly laid concrete transversely to the direction of the road.

This method was originally developed to remove any unevenness in the surface, but it really accomplishes a very much more important result as it squeezes out of the concrete much of the excess water.

In order to determine the effect on the strength of a concrete produced by this finishing method above described, a number of slabs were made for testing purposes, mixed and molded in much the same manner as in concrete road work, the exact proportions of water and other materials being recorded. The principal variation was in the consistency of the concrete and the method of finishing the surface.

The tests were made in cooperation with Prof. D. A. Abrams, in charge of the Structural Materials Research Laboratory, Lewis Institute. Professor Abrams gave his personal supervision to all of the details of the tests.

Twelve slabs, 2½ ft. wide, 5½ ft. long and 5 in. in thickness, were made and tested. Pebbles were used as coarse aggregate for six of these and crushed limestone was used for the others.

The concrete was mixed in the proportion of 1 part cement to 4 parts total mixed aggregate by volume—a proportion approximately equivalent to the ordinary 1 : 2 : 3 mix. Three sets of slabs, two slabs in each set, were made from both crushed limestone and pebbles as a coarse aggregate.

The following consistencies of concrete were used in the test:

1. Dry consistency, finished with a wood float in the ordinary way;
2. Wet consistency, finished with a wood float in the ordinary way;
3. Wet consistency, finished by means of a roller, as previously described.

The slabs were made on the concrete floor of the laboratory, the form being placed on a sheet of building paper.

The concrete for the slabs of dry consistency was mixed as stiff as could be easily handled—stiffer than is ordinarily found in concrete road work. The mixtures for the remaining slabs contained a somewhat larger amount of water, making a consistency that would ordinarily be classed as "good" in practical work. (See Table I.)

All slabs of the stiffer consistency, as well as one of the slabs of each other set, were finished by being first struck off with a straight edge and then floated by hand with a wooden float. The remaining slabs of each of the wet consistency sets were finished by the use of a roller. The concrete was left a little more than flush with the sides of the mold, and instead of being struck at once with a straight edge, it was first rolled; the straight edge was then used to strike off what surplus concrete remained. To bring the full weight of the roller—which was just long enough to rest upon the side forms—on the concrete the slab was rolled transversely at intervals of about 15 minutes for $1\frac{1}{2}$ hours, five rollings being made. The roller was moved back and forth over the width of the slab, moving sidewise a few inches with each stroke. As each rolling was made, considerable water was squeezed out of the concrete and forced over the sides of the mold. The rolling continued until the amount of free water became very small. No particular difference in the amount of rolling to effect this was noticeable for the limestone concrete and the gravel concrete. It was noticed, however, that the roller sank into the gravel concrete considerably more than into the rock concrete during the first one or two rollings.

The roller used was $2\frac{1}{2}$ ft. long, 6 in. in diameter and weighed about 50 lb. In practical road work, an 8-in. roller about 6 ft. long, weighing about 70 lb., is recommended.

The forms were removed the following day, when the slab was covered with a layer of damp sand which was kept moist until the slabs were one week old. The sand was then removed and the slabs lifted from the floor and permitted to dry out during the remaining eight days before the load was applied.

The arrangement for applying the load to the slabs is shown in Fig. 1. The slabs were placed on two supports spaced 15 in. on centers, resting on the floor. A ball bearing on the lower side of one support compensated for what warp there might be in the lower surface of the slab. The load, which consisted of 100-lb. sacks of sand, was supported by two 9-in. I-beams, and was transmitted to the slab through two bearings spaced 5 ft. apart. The upper surface of the slab was thus put in tension. Each test was made in the same manner, and as far as practicable the loads were applied at the same rate until rupture.

The appearance of the broken slabs disclosed a marked difference between those finished with the roller and those finished with the wooden float. The roller-finished slabs showed

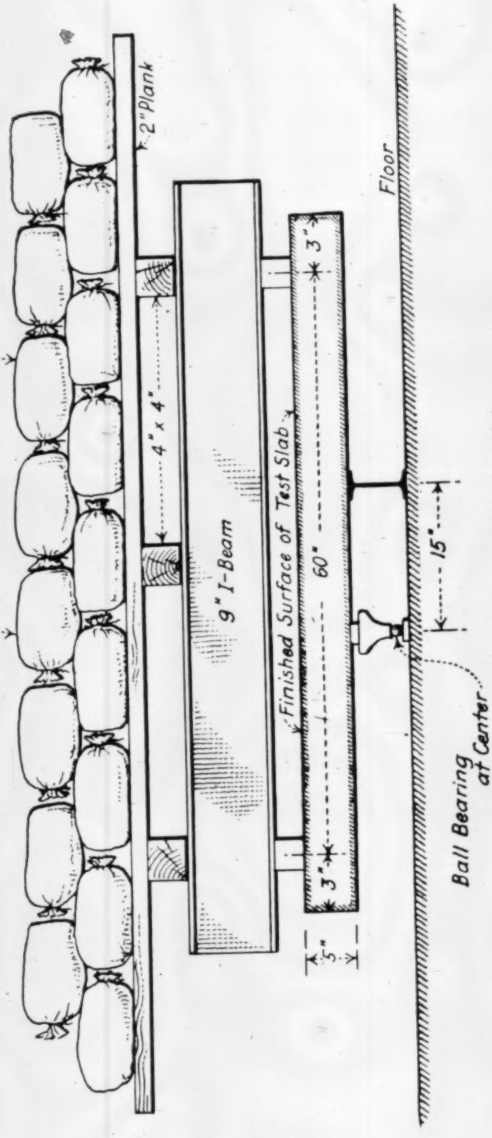
100 lb. Sacks of Sand
2" Plank
4" x 4"
9" I-Beam
Finished Surface of Test Slab
Floor
3"
60"
3"
5"
15"
Ball Bearing at Center

FIG. 1.—Method of Testing Slab.

a distinctly denser appearance, extending for at least one-half the depth of the slab. It is to be noticed (see Table I) that the slabs tested at an age of 16 days show a consistent increase in strength over those 15 days old.

With the small number of test specimens, there are not sufficient data to determine definitely any special difference between the strength developed by the slabs made of pebbles and those with the crushed limestone. It is to be noted, however, that the pebbles used were somewhat irregular in shape, some pieces being broken.

TABLE I.—RESULTS OF TRANSVERSE TESTS OF CONCRETE SLABS FINISHED BY WOOD FLOAT AND BY ROLLER METHOD.

Concrete was machine-mixed in the proportion of 1 part cement to 4 parts aggregate, by volume. Size of slabs, 30 by 66 by 5 in. Loaded with dead weights at points 15 in. apart on 5-ft. span. Duplicate slabs were made on different days.

Specimen No.	Age at Test, days.	Total Applied Load, lb.	Thickness,[1] in.	Kind of Coarse Aggregate.	Water Used, Percentage of Volume of Aggregate.	Modulus of Rupture of Concrete, lb. per sq. in.			Compressive Strength of Concrete (6 by 12-in. cylinders), lb. per sq. in.
						Medium Consistency, Wood-Float Finish.	Wet Consistency.		
							Wood-Float-Finish.	Roller Finish.	
1	15	3300	5.20	Pebbles	15.9	301	...	...	....
1a	15	3200	5.14	Pebbles	15.9	302	...	...	2370
2	15	3200	5.18	Pebbles	18.6	...	294	...	1760
2a	15	3500	5.27	Pebbles	18.6	...	311	...	1730
3	15	3850	5.32	Pebbles	18.6	...	...	331	1680
3a	15	3400	5.07	Pebbles	18.6	...	...	325	1680
4	15	4000	5.25	Crushed Limestone	15.9	353	...	...	2130
4a	16	4500	5.18	Crushed Limestone	15.9	405	...	...	2290
6	15	3000	5.11	Crushed Limestone	18.6	...	288	...	1600
6a	16	3750	5.20	Crushed Limestone	18.6	...	340	...	1680
5	15	3650	4.87	Crushed Limestone	18.6	...	...	378	1330
5a	16	4950	5.17	Crushed Limestone	18.6	...	...	443	1800
Average.	......	....	......	....	....	340	308	369	

[1] Thickness of the slabs at the broken sections.

The broken sections of all slabs, particularly those that were rolled, showed that many pieces of the coarse aggregate had been broken apart, the number of broken pieces being considerably greater in the rolled slabs than in those not rolled. There was no difference especially noticeable in this particular, between the slabs made of the limestone and those made of the pebbles.

The results of the tests are shown in Table I. The noticeable fact is that those slabs finished with the roller developed a very considerable increase in strength over the slabs merely hand-finished. The four slabs of wet consistency that were finished by hand, have an average modulus of rupture of 308 lb. per sq. in., while the four slabs finished with the roller have an average modulus of rupture of 369 lb. per sq. in., or an increase of almost 20 per cent. As would be expected, the slabs that were made of a stiffer mixture, indicated in Table I as medium consistency, giving an average modulus of 340 lb. per sq. in., are stronger than those of the wet consistency, finished in the same manner; but the wet-consistency slabs finished with the roller are stronger than those of the medium consistency, showing an increase of nearly 10 per cent.

The results seem to indicate clearly the value to be gained by the use of the roller to finish a concrete road; that it is possible by proper manipulation of concrete to secure increased strength and density of a character most desirable for a concrete road surface; and that such surplus water as may be required to facilitate placing concrete in road work can be effectively removed by this simple expedient.

DISCUSSION.

Mr. Mattimore.

Mr. H. S. Mattimore.—Regardless of the increase of strength, the manipulation described by Mr. Johnson accomplishes just what we would expect by dehydrating the concrete and getting a drier mix. Another big feature which Mr. Johnson has not brought out, and which I think is the best feature of all, is that of *transverse* rolling. By the old screening method, longitudinal with the road, irregularites were left which were all transverse to the direction of traffic and therefore objectionable to moving vehicles. In rolling transversely any irregularities are in line with the traffic, and therefore less objectionable.

Mr. Wig.

Mr. R. J. Wig.—Is it proposed that a wet mixture can be used without affecting the quality of the concrete if the roller is subsequently employed?

Mr. A. N. Johnson.

Mr. A. N. Johnson.—I would not say that. I should say that within the ordinary variations of what we consider a fair mixture—not a sloppy, soupy mixture, but a fairly thin mixture—the concrete is improved by the use of the roller. There are other methods used for finishing concrete surfaces that produce similar results, that is, they have the effect of so agitating the surface of the concrete as to take out the excess water. Of all these methods, the one which I have described is the simplest and seems quite as effective as any.

Mr. Bates.

Mr. P. H. Bates.—The author calls attention to the fact that there is quite a "consistent increase in strength," as he says, shown by those specimens tested at 16 days over those tested a 15 days. Upon examining the data in Table I, one finds a remarkable gain in strength for one day's longer storage. For instance, the 6 by 12-in. cylinder, specimen No. 5, tested at the end of 15 days shows a compressive strength of 1330 lb. per sq. in.; tested one day later, specimen No. 5*a* shows a compressive strength of 1800 lb. per sq. in., a gain of 35 per cent by one day's longer storage. The modulus of rupture is increased almost 20 per cent.

It seems to me that is a considerable gain in strength for one

Mr. Bates. day's storage. It would seem hardly possible that the test pieces were similar test pieces. You cannot obtain an increase in strength of concrete of over 30 per cent between the fifteenth and sixteenth days; you can hardly get that between the seventh and twenty-eighth days.

Mr. Kinney. MR. W. M. KINNEY.—It seems to me that Mr. Bates has not taken into consideration all of the results. Take the cylindrical specimens Nos. 6 and *6a*, Table I; at 15 days, No. 6 shows a compressive strength of 1600 lb. per sq. in., and No. *6a*, at 16 days, a compressive strength of 1680 lb. That is a reasonable increase. Specimen No. 4, at 15 days, shows a compressive strength of 2130 lb. per sq. in.; specimen No. *4a*, at 16 days, shows a compressive strength of 2290 lb., which is a reasonable increase. Frequently in concrete testing a difference of four or five days in the time of making the test pieces may make a considerable difference in the strength obtained.

Mr. Bates. MR. BATES.—But according to this table we have in specimens Nos. 5 and *5a*, a difference in modulus of rupture of nearly 20 per cent in one day. What I cannot understand is how such a wonderful increase in strength can be obtained in one day. Sub-Committee VII on Strength, of Committee C-1, never found any such facts in all their work, and they have broken a great many specimens.

Mr.A.N.Johnson. MR. A. N. JOHNSON.—There is perhaps one feature that might explain the increase in strength referred to by Mr. Bates, namely, the fact that the specimens were subjected to moisture for only seven days and the remainder of the time were drying out in the atmosphere of the laboratory. This probably accounts for some additional increase other than what would be expected.

Mr. Wig. MR. WIG.—I think Mr. Bates has overlooked the fact that each figure in Table I represents the result of test of only one specimen. In the case of specimens Nos. 1 and 1*a* and 2 and *2a*, although there is a difference of nearly 3 per cent in the amount of water used in mixing, the values of modulus of rupture obtained are nearly identical. That, I am sure, is contrary to the results obtained in most laboratories.

Mr.A.N.Johnson. MR. A. N. JOHNSON.—The conclusion to be drawn from the tests is that the rolled sections are invariably stronger than those that are not rolled. That was the only point I set out to establish, and that is well borne out by the results.

Mr. N. C. Johnson.

MR. NATHAN C. JOHNSON.—In trying to sense the spirit and trend of this session on "Concrete," as well as that of meetings of related committees which I have attended, there is uppermost in my mind an impression of uncertainty and confusion; and in view of the unusual lack of discussion, the inference seems fair that a like sense of confusion exists in the minds of others as well.

An outstanding feature of committee reports and papers on concrete is, that results obtained by different investigators in the same field are often contradictory; and the various analyses of these results show a growing understanding that the concrete art is as yet hap-hazard. Mr. Enright in his communications has touched upon one point which is vital, in that he has brought to our notice, if we had forgotten it, that physical testing is not by any means an exact science.[1] Necessarily this inexactitude imposes broad limitations on the value to be attached to researches which depend wholly or to a large extent upon physical testing. Furthermore, we are unable practically to make use of even the limited value of these researches save in isolated and accidental reproductions of test conditions, in that we unfortunately have no standard procedure to conform to in actual concrete construction, either in selection or measurement of materials or in methods of manufacture. Cement makers use every effort to produce one component of concrete which shall be standard, but this one material, small in quantity as compared with the bulk of other and uncertain materials, is turned over for final manufacture to the mercies of a gang of laborers who will supply any further factors needed to insure a uniformly poor quality. Nor are engineers to be held blameless. The average inspector has little better knowledge, and that this lack of understanding exists is largely due to complaisant acquiescence in the easiest way by those who should take a different attitude. All concrete is not good concrete, and we are slowly coming to a realization of that fact.

I believe, however, that this meeting and the meetings of the committees relating to concrete show that we are making progress. Mistakes are powerful correctives. Results formerly given in text books with beautiful curves showing strengths at

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XVII, Part I, p. 198 (1917).

Mr. N. C. Johnson. 7 and 28 days, and one year, and so on, are being largely disregarded in so far as their application to field concretes is concerned. The application of the so-called "laws of concrete" shows such eccentric and fantastic exceptions as to properly bring into question their right to the title. To-day no man can predict the product that will be obtained with given materials when they are mixed together as concrete, the method of their admixture and placement being left to individual discretion; and with variable materials, the results are even more uncertain.

So long as these facts were denied, with special denials from those highest in authority, the outlook was not encouraging. But with their recognition, there is new hope. The realization of a need is the first step in its fulfillment. Lack of comment in these usually critical meetings reflects the realization of needs, but uncertainty as to how the ends shall be attained. For that reason, the thoughtful repression so generally noticeable at this meeting is an encouraging sign of progress.

Mr. Edwards. MR. L. N. EDWARDS.—With reference to the question of the strength of the cylinders, a considerable variation in strength is frequently found in the testing of cylinders made from a single batch of concrete. This is mainly due to irregular distribution of the stone aggregate, and to the location of the stone particles in such position as to produce planes of cleavage, or other conditions of structural weakness.

In these days of good road construction, the paper presented is of interest. In discussing it I merely wish to call attention to another method of securing the same results, that is, the compacting of the concrete and the removal of a portion of the water content of the mix. Last year, I constructed about 1400 sq. yd. of concrete roadway—not a great deal, to be sure—forming an approach to one of our bridges then under construction in Toronto. After the usual preparation of the foundation, the concrete forming the roadway slab was placed in two layers, practically simultaneously. The base or first layer, 4 or 5 in. in thickness, was placed a few feet in advance of the second or surface layer. In the base layer 2-in. broken stone was used, and in the surface layer 1-in. stone. The latter was also richer in cement than the base layer. Only a sufficient amount of water was used to produce a sticky semi-plastic mortar. Immediately following the placing of the surface layer, the top surface was screeded to the desired shape, and was then floated with a heavy wooden float weighing about one hundred pounds, operated by two men. The first operation of floating consisted in raising and dropping the float, thus forcing the particles of stone aggregate below the surface, compacting the concrete and producing a dense mortar surface. This was followed by a thorough floating of the surface, produced by giving the wooden float a see-saw movement combined with a forward movement. Following the floating, a piece of 1½-in. plank, about 10 in. wide by 3 ft. long, attached perpendicularly to a convenient handle, was made to produce a series of irregular ridges upon the mortar surface by being lifted vertically and suddenly out of contact with the mortar. The object of these ridges is to produce an irregular surface to prevent horses and vehicles from slipping. Considering the consistency of the mix, the amount of water forced to the surface throughout the floating and surfacing operations was considerable. I believe that upon work where large areas are involved, this method would prove quite as efficient and economical as that described by Mr. Johnson. **Mr. Edwards.**

Somewhat similar methods of floating the top surface of the concrete have been used, but I am not aware that in any case the concrete has been placed in two layers.

Mr. Abrams. MR. D. A. ABRAMS.—These tests that Mr. Johnson described were made in the laboratory with which I am connected, consequently I feel somewhat responsible for his embarrassment with reference to the questions about the changes in the strength of the concrete. Now, as a matter of fact, every one knows that if you take concrete out of one batch from a mixer and make a test piece, and two or three days later take another sample out of another batch of concrete, which is presumably the same, and make another test piece, you will not get concrete of exactly the same strength. So much for the machine-mixed concrete. We do not use machine-mixed concrete in our research work. All concrete in our research work is mixed by hand, each specimen being made from a separate batch which is proportioned to the exact quantity required.

Now if there is any question of uniformity in tests, I should

Mr. Abrams. like to refer to Table III of the paper which I presented, on the "Effect of Rate of Application of Load on the Compressive Strength of Concrete."[1] An examination of this table will indicate what can be done with hand-mixed concrete in making compression tests on concrete specimens on different days, using exactly the same materials. Both Mr. Bates and Mr. Wig in their discussion brought out, very properly, the fact that concrete mixed in a machine is not as uniform as we should like and is by no means as uniform as you can get by standard methods of hand-mixing. Uniformity is quite essential in the case of such work where we want to get at the exact properties of the material and study the effect of certain variations in manipulation, etc. The mean error of these tests made on different days ranges from 4 to 8 per cent, the latter being the value of the very lean mixtures. We always find that the lean mixtures give larger errors than the richer ones. I do not believe there can be any proper objection raised to test data that show no greater variations than 4 to 8 per cent. We are ready to grant that machine-mixed concrete permits of considerably wider variations in the results of tests made on specimens from different batches.

To pursue this question of variation in test results a little further: I have been making a study, as I found opportunity, of the variation in the results of physical tests of different materials. After studying the results of tests of a large number of materials, including iron, steel, zinc, copper, timber, brick, and other terra cotta ware, I have arrived at the conclusion that we can make concrete test pieces from the same materials on different days with a greater uniformity than is shown by the results of tests on any other material except mild steel.

Mr.N.C.Johnson. MR. NATHAN C. JOHNSON.—I am impelled by this discussion on the strength of specimens to inquire what compressive strength *per se* has to do with the durability of concrete roads. A properly drained and foundationed concrete road should not function as a beam, yet we commonly recommend reinforcing it with from 0.1 to 0.3 per cent of steel and when such proportions prove inadequate we carry the reinforcement as high as 1 per cent.

With proper foundation, a concrete road would seem to demand an abrasion and impact resisting surface with sufficient strength and mass to resist concentrated strains and impact. **Mr. N. C. Johnson.** With proper abrasion-resisting materials firmly imbedded in mortar, there should not be required steel reinforcement, substantially for the purpose of keeping the road from cracking itself to pieces.

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[1] See p. 370.—ED.

Mr. Johnson has proposed in his paper an empirical and somewhat questionable method of eliminating one fault at the expense of losing a certain proportion of dissolved cementing materials. There are other and better ways of securing plasticity than by excess water. The root of the question has not been touched nor the desired goal brought any nearer by this method. A concrete pavement should properly be a wonderful pavement, but it is doubtful if it will attain its full usefulness until palliatives are abandoned and proper methods substituted and rigidly insisted upon. Not least of these is such preparation of the sub-soil that irresistible frost movements shall not be permitted to impose strains on the concrete which it should properly not be called upon to bear.

Mr. Thompson. MR. SANFORD E. THOMPSON.—I do not quite understand the last speaker's criticism of Mr. Johnson's paper. It is true that poor methods are apt to be used in making concrete roads, and it is well to call attention to those defects. At the same time, such treatment as Mr. Johnson suggests is of value not only in removing excess water but in increasing the density of the surface, thus making it higher in strength, more resistant, and more waterproof.

Mr. Spackman. MR. H. S. SPACKMAN.—I have been impressed with one point in this discussion, namely, the elimination of excess water. There is an old saying, "Prevention is better than cure." Why is it necessary, in concrete road construction, to use the excess water at all? Why cannot a dry mix be used, unless it is the intention to make the water do the work of the laborer?

Mr. A. N. Johnson. MR. A. N. JOHNSON.—There is a practical objection to using a very stiff mixture on a concrete road and that is the difficulty met with in striking the concrete. The use of a rather more plastic mixture aids materially in shaping and striking the road.

I should like further to state that a good many miles of concrete roads finished as described in my paper, which I have had

Mr.A.N.Johnson. the opportunity of observing, are so markedly superior to the average concrete road that I was led to inquire into the cause. When I saw the work in operation, the quantity of water pressed out of the road, and knowing the benefit of such treatment, it occurred to me to make a series of tests merely to express that fact through some fairly definite figures. I knew the benefit was there; these tests are merely a laboratory confirmation of what had been observed.

Mr. Hough. MR. NORMAN G. HOUGH.—In connection with Mr. Spackman's remark that "prevention is better than cure," it would seem to me that this is the proper line along which to work. The excess water ought not to be placed in concrete if it is necessary, immediately after placing, to take it out by rolling.

Mr. Johnson says that it is often necessary to use excess water to bring the mixture to an easy working condition—to make it plastic. I wish to call attention to the fact that a number of highway engineers are securing the necessary degree of plasticity in the construction of concrete roads by the use of small percentages of hydrated lime, and when lime is used it is unnecessary to add an excess of water. The fact that they are getting good results is evidenced by their continued use of lime. It would seem to me that if hydrated lime introduces this greater degree of plasticity without employing excess water, it is worthy of consideration, as the rolling is done, apparently, only to bring out the excess water. It is well known, and has been shown by laboratory tests, that excess water greatly reduces the strength of concrete, and it would seem, therefore, that the use of hydrated lime in this connection must be considered of great practical value.

Mr. Slater. MR. W. A. SLATER.—I should like to ask whether, in rolling the road, some of the cement is drained off with the water. However, I suppose that the "proof of the pudding" is, in this case, in the testing; that is, if the slabs showed that the modulus of rupture was actually greater for the rolled specimens than for those that were not rolled, that should settle the question I have asked.

Mr.A.N.Johnson. MR. A. N. JOHNSON.—Practically no cement comes off with the water. The concrete is allowed to stand for a few moments, the water collects on the top, the cement sinks to the bottom, and in rolling the water off, there is not sufficient wash or current created to take away any appreciable amount of cement. Moreover, the rolled sections have a denser surface. That is really the value of rolling—making the surface dense. Whether hydrated lime is added, or any other method is adopted that permits the use of a less amount of water, it would still be an advantage and almost a necessity to finish the road in some such manner to close up the pores as the strike board displaces the surface aggregates. The fact is that rolling, as well as some other methods of finishing the road, has the effect of laying the surface aggregate back in place and thoroughly compacting the mortar around it, thus increasing the density of the surface. **Mr.A.N.Johnson.**

MR. W. M. KINNEY.—I have never seen the roller worked practically, but have recently observed the use of a long handled float which works in much the same way. Almost clear water came down into the gutter, pushed forward by the long handled float. With regard to Mr. Spackman's question, we all know that if mortar or concrete is stiff it is more difficult to work than if it is mushy. We have the same condition in using too dry a mixture in concrete roads. There is a tendency to pull out stones and form cracks in the surface which are rather difficult to seal together in later work. If the mixture is too stiff, the workmanship is more difficult and the finish not so good. We must therefore use a wetter mixture, and some scheme is necessary to remove the excess water, such as the one suggested in this series of tests, or another one used by P. M. Bruner, a very good floor builder in St. Louis, who lays down a section of burlap on the fresh concrete and absorbs the excess water by means of another layer of cement-sand mixture. This does not mean that we have to use a great excess, but just enough to give a workable concrete. **Mr. Kinney.**

TESTS OF CONCRETE ROAD AGGREGATES.

By J. P. Nash.

SUMMARY.

The two causes of deterioration of concrete for public thoroughfares are the wearing action of traffic and the cracking of the concrete, due to its weakness in tensile strength.

In this paper are described tests made on concrete in which various coarse aggregates were used. The test specimens to determine resistance to wear consisted of concrete rings, which were revolved in a standard brick rattler, the loss in weight and appearance of the surface of the specimens being the index of the wearing qualities of the aggregate. An apparatus was devised to test the concrete in tension. The results of these tests, together with compression tests of the same concrete, are presented in tabular form.

The two most important essentials for a satisfactory road are first, uniformity of wear; second, a minimum of wear. Whenever these two are combined with a high tensile strength, the most satisfactory concrete road is obtained.

The conclusions drawn from these tests are as follows:

1. Uniformity of wear is obtained when the mortar and the coarse aggregate wear equally, such as when crushed limestone or limestone gravel is used in a 1 : 2 : 4 mix.

2. The coarse aggregate should be limited in size to about 1½ in.

3. When hard, tough stone is used, the size should be limited to about 1 in. and the cement content increased.

4. It is questionable if a richer mix than a 1 : 2 : 4 is an economical one to use with crushed limestone of the ordinary hardness. Crushed slag when hard and uniform should be satisfactory as a concrete road aggregate from the standpoint of wear.

5. In a 1 : 2 : 4 concrete, a gravel composed of very hard stones such as flint, or quartz, does not wear uniformly.

6. The action of the cubical shot on the test specimens is a trifle more severe than the traffic on the road.

7. It cannot be said that either the crushed stone or gravel tested is superior as an aggregate to produce concrete having a higher tensile strength.

TESTS OF CONCRETE ROAD AGGREGATES.

By J. P. Nash.

With the increasing mileage of concrete roads constructed each year, comes the pertinent question as to the most desirable materials to use in their construction, and also the proper methods of mixing, laying and curing the concrete. The effects of the latter variables, namely, the mixing, laying and curing, are fairly well known; but the wearing qualities under continued traffic for a period of years has not been fully demonstrated, as few concrete roads have been in existence as long as ten years. Most of the properties which a concrete for road construction should possess are the same as it should have for any other purpose, with one additional property: it must resist the abrasive action of traffic. The tensile strength also plays an important function in a concrete road, as it is that property which largely determines the number of cracks

The resistance to abrasion is determined by the tenacity of the aggregates to hold together under impact and friction, and also by the resistance to wear of the separate aggregates. The resistance of the aggregates to separation is a function of the cement, and the cleanness and grading of the aggregates, while the resistance of the aggregates to wear is an inherent property of the aggregates themselves.

By far the greater proportion of cracks in a concrete road are contraction cracks. When the material contracts, tensile stresses are set up due to the friction of the concrete on the sub-base. The road cracks when this friction is greater than the tensile strength of the concrete slab. Other things being equal, the concrete having the highest tensile strength will have fewest cracks.

Object of the Tests.

The object of the tests described in this paper was to determine the resistance to abrasion and the tensile strength of concrete made with various aggregates. In these tests the chief variable was the coarse aggregate. The sand used in most of the tests was a well-graded Colorado River sand, passing a $\frac{1}{4}$-in. sieve. However, where the coarse aggregate was graded to $\frac{1}{8}$ in. a finer sand was used. The Colorado River sand is composed of rather angular fragments of flint, quartz, and a small proportion of limestone. With the exception of a slag from Birmingham, Alabama, and a sample of gravel from New Orleans, all the materials used were from Texas. Similar materials may be found in most parts of the United States.

The writer desires to express his appreciation of the excellent work done by Prof. Charles F. Shoop of the University of Minnesota, along the same line as this. The specimens used by the writer are of such a size that the results may be compared with those found by Professor Shoop.

DESCRIPTION OF THE TESTS.

Making the Specimens.—The amount of material on hand to make the tests was meager. The samples were sent to the laboratory by producers, who in no case furnished the full amount requested. The number of test specimens made, consequently, had to be limited in number to one for the abrasion test, one for the tension test, and, in most cases, only two for the compression test.

The concrete was mixed on the floor, with shovels, to a medium consistency and tamped in the molds with a $\frac{5}{8}$-in. round steel bar. After 24 hours, the specimens were removed from the molds and placed in wet sand for 6 days, when they were removed and stored in the laboratory without further moistening, until tested at the end of 28 days.

The molds used for these tests were made of No. 16 galvanized sheet iron. Later, cast-iron molds were procured. These two types of molds are shown in Fig. 1.

The compression specimens were 6 in. in diameter and 12 in. high, made on plate glass so that a plain surface was secured on one end, the other being bedded in plaster of Paris when tested.

The tension specimens were similar in shape to the standard cement briquettes, having a sectional area at the center of 25 sq. in. (Fig. 2). The specimen was 5 in. deep and at the

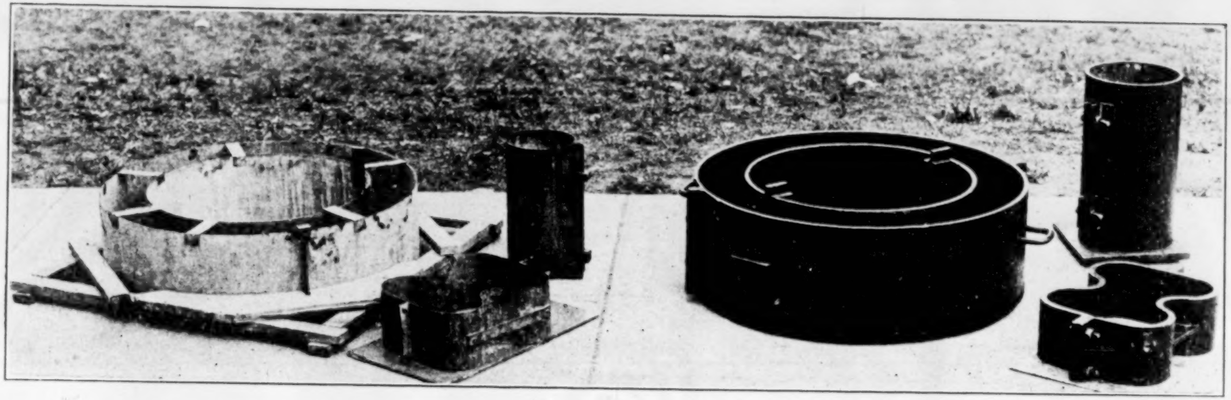


FIG. 1.—Molds Used for Specimens on Left. Improved Cast-Iron Molds on Right. Lighter Molds are Excellent for Field Use.

narrowest part was 5 in. wide, while the length was 12 in. With a specimen of this type the concrete may be placed in the mold in the same way in which it is placed on the road. The line of stress is also the same in both cases.

The abrasion specimens consisted of a large concrete ring 20 in. in inside diameter, 28 in. in outside diameter, and 8 in. deep (Fig. 2). The 4-in. thickness of concrete was reinforced with two steel rods to prevent cracking from unequal stresses in the rattler. The specimen was molded between two concentric metal forms held in place by metal spacers as shown in Fig. 1. This specimen can be handled by one man without the assistance of any apparatus, the average weight being about 200 lb.

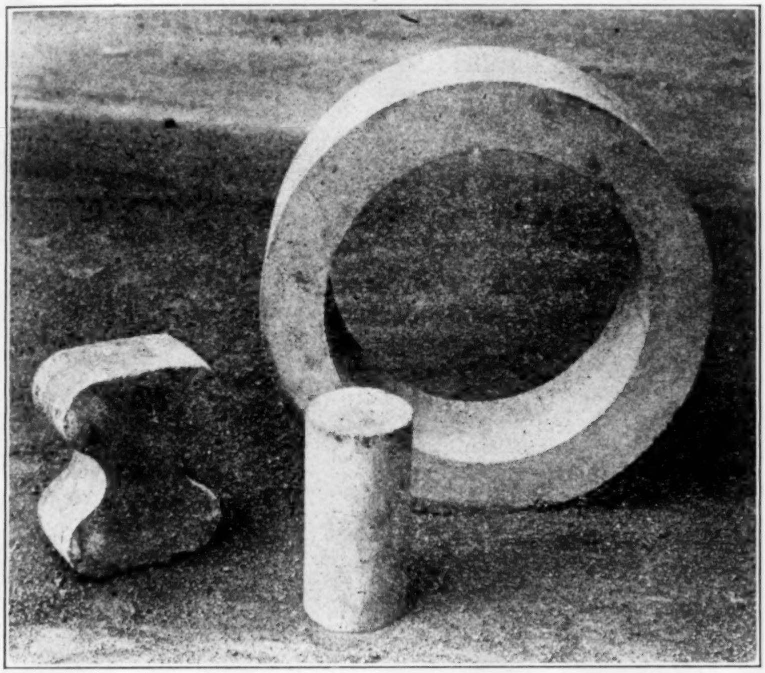
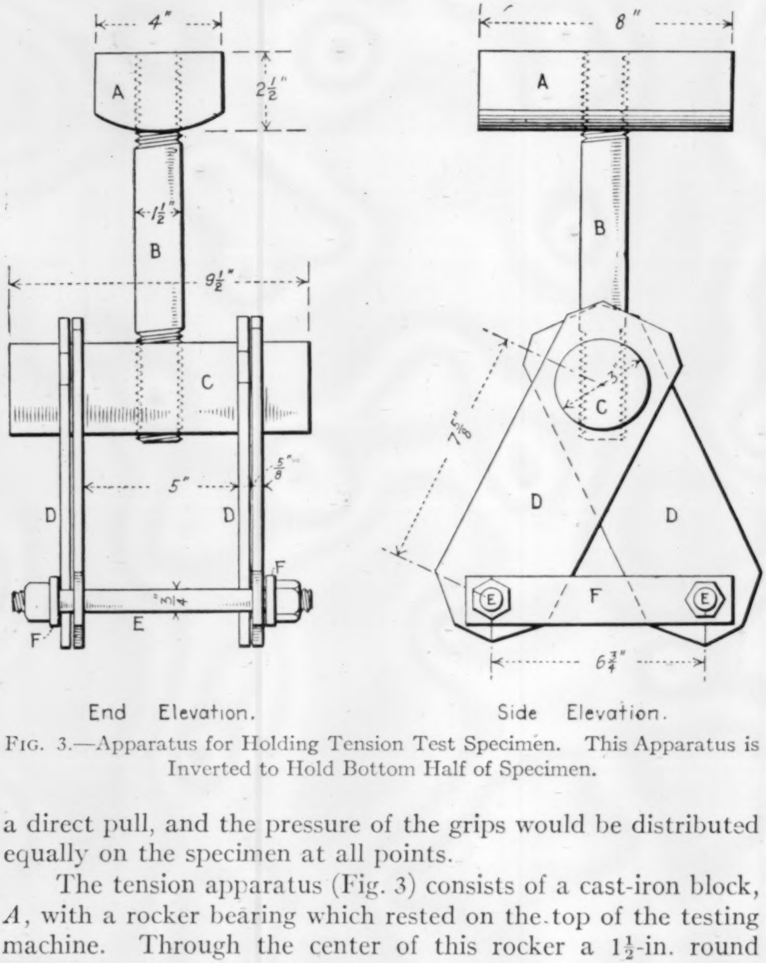


FIG. 2.—Specimens Used in Tests (about $\frac{1}{12}$ Natural Size).

Testing the Specimens.—In testing the compression and tension specimens a 100,000-lb. Olsen testing machine was used, the load in both cases being applied at a speed of 0.05 in. per minute. One end of each compression specimen was bedded in plaster. In testing, the spherical bearing-block was placed at the top. Grips were devised for the tension specimens so that when the load was applied, the specimen would adjust itself to a direct pull, and the pressure of the grips would be distributed equally on the specimen at all points.



End Elevation. Side Elevation.

FIG. 3.—Apparatus for Holding Tension Test Specimen. This Apparatus is Inverted to Hold Bottom Half of Specimen.

The tension apparatus (Fig. 3) consists of a cast-iron block, *A*, with a rocker bearing which rested on the top of the testing machine. Through the center of this rocker a 1½-in. round bar, *B*, is screwed, which is also screwed into another cast-iron block, *C*. This makes it possible to take the apparatus off the testing machine. The ends of this lower block are turned true in the lathe and two flat bars, *D*, are hung from each end. This leaves a clear space of 5 in. between these two pairs of bars, allowing the specimen to hang between them. Through a hole in the lower ends of these bars, two $\frac{3}{4}$-in. round bars, *E*, are placed at right angles. These bars serve as bearings for the specimen. Side spacing bars, *F*, hold them apart, while the specimen hangs on the $\frac{3}{4}$-in. round rods. This arrangement was

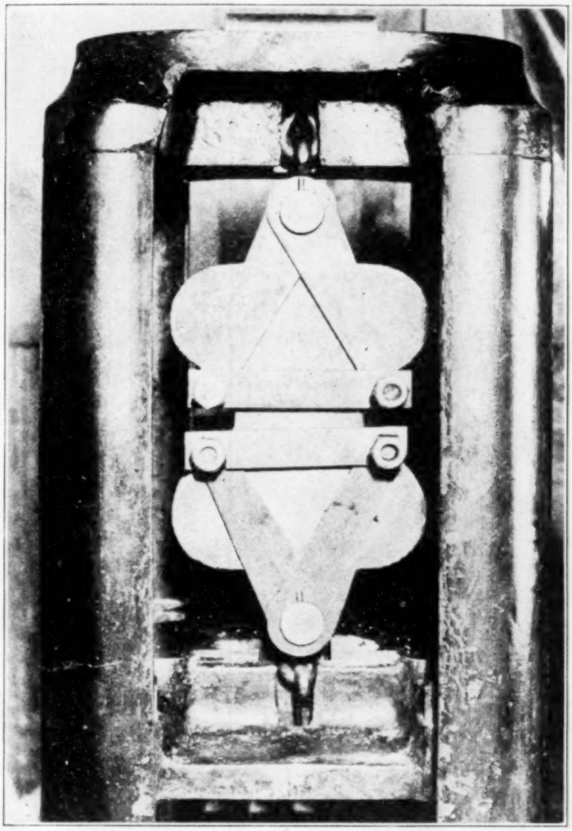


FIG. 4.—New Type of Apparatus for Tension Test of Concrete.

duplicated in inverted position from the moving head of the machine, which served to grasp the lower half of the tension specimen.

The apparatus was designed to test any specimen up to the capacity of the machine and it was found that it was somewhat unwieldy. A lighter apparatus, shown in Fig. 4, was designed later. This will test specimens up to 20,000 lb. total load, which is ample for any concrete that it is possible to make. The direct tension is assured with the apparatus by a welded eye from which the clip hanger allows a limited universal motion.

In order to determine what variation might be expected in the results of the tension tests, four independent series of tests were made, each series consisting of three specimens made on different days. An exception to this was the three tests made on the Ottawa-sand briquettes, which were all made from the same batch. A different mix was used for each series. The results obtained from these series are given in Table I. From these results it is reasonable to assume that a single tension test would fall within 10 per cent of the average. Results on other tests, not included here, indicate that the probable error on concrete specimens is much smaller, being closer to 5 than 10 per cent. The specimens were tested at the rate of 40 lb. per sq. in. per minute.

TABLE I.—RESULTS OF AN INDEPENDENT SERIES OF TENSION TESTS.

Mix.	Tensile Strength, lb. per sq. in.	Maximum Variation from Average, per cent.
1 Cement : 3 Ottawa Sand	202, 220, 216..Average 213	5.2
1 Cement : 3 Colorado River Sand	264, 263, 294..Average 274	7.3
1 Cement : 2 Colorado River Sand : 4 Colorado River Gravel	275, 274, 275..Average 275	0.4
1 Cement : 1½ Colorado River Sand : 3 Colorado River Gravel	288, 304, 280..Average 291	4.0

In order to determine how closely the large 5-in. briquettes could be expected to check with the smaller 1-in. standard briquettes, some 1 : 3 Ottawa-sand mortar was made into briquettes of both sizes at the same time. The speed of the small tension testing machine was regulated to that of the large machine so that the rate per square inch of load would be the same. It was found that the smaller briquettes tested 12.4 per cent higher than the larger briquettes when tested with the old apparatus, shown in Fig. 3, and 2.1 per cent higher when the large briquettes were tested with the new apparatus, illustrated in Fig. 4. It is believed, however, that this difference is due to the variations that might enter in making and curing the specimens, rather than in the apparatus. For instance, the mortar tested with the old apparatus was mixed with shovels on the floor, while that tested with the new apparatus was kneaded by hand, according to standard methods.

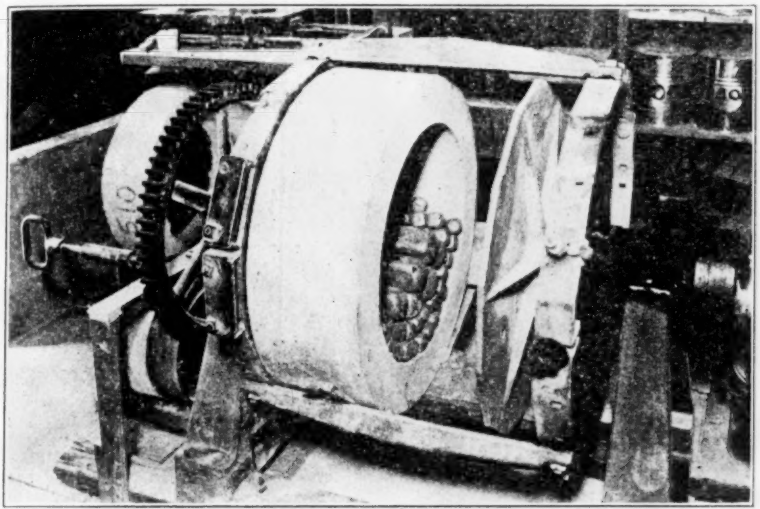


FIG. 5.—Abrasion Test Specimen with Shot in Position in Rattler.

The abrasion test on the concrete rings was made in the standard brick rattler, having alternate staves removed (Fig. 5). This was done in order to lighten the load on the pulley. The axis of the specimen was aligned with the axis of the barrel by driving wooden wedges between the staves and the specimen. After the shot were placed in the ring the moving head of the barrel was brought in contact with the specimen, holding it in place. The rattler revolved at the rate of 30 r.p.m., which was sufficient to cause the shot to ride up the specimen while it revolved This permitted some of the shot to slide upon the concrete while others dropped from the top of the pile with a certain amount of impact. The charge used consisted of 139 cast-iron cubes measuring $1\frac{1}{2}$ in. with rounded edges. In addition, there were six rectangular shot $2\frac{5}{8}$ by $2\frac{5}{8}$ by $4\frac{1}{2}$ in. which brought the total weight of the charge up to 158 lb.

After each 500 revolutions the specimen was cleaned of all dust and weighed, in order to determine what effect the number of revolutions would have on wear. It was found that the specimen lost approximately the same amount each 500 revolutions. The specimen was tested for 2000 revolutions, but the percentage loss was calculated for 1000 revolutions. The character of the surface of the specimens was carefully noted after each test. These observations furnish some of the most important features of these tests.

TABLE II.—ABRASION TESTS OF THREE SPECIMENS.

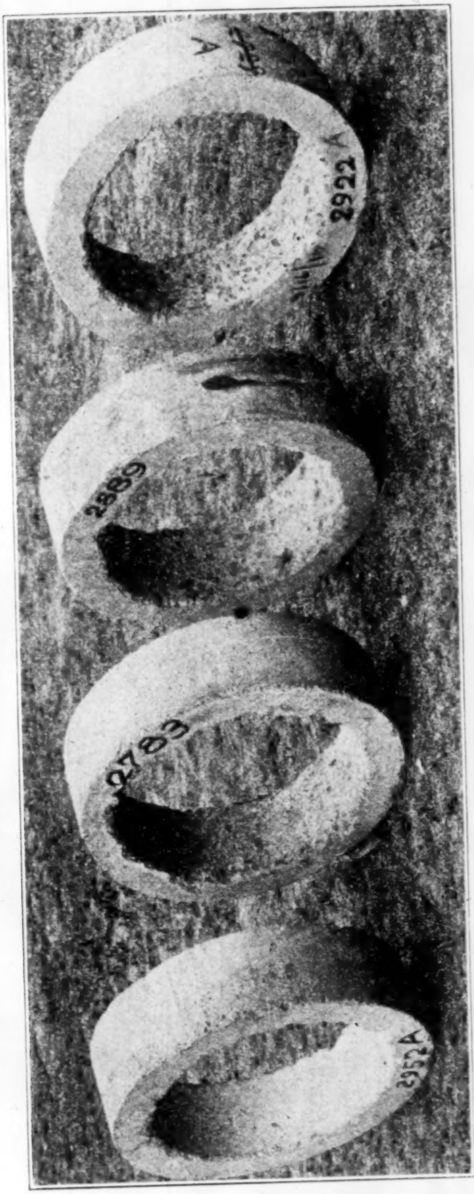
1 PART CEMENT : 2 PARTS COLORADO RIVER SAND : 4 PARTS COLORADO RIVER GRAVEL.
AGE 28 DAYS.

First specimen lost	3.81	per cent by weight in 1000 revolutions
Second specimen lost	3.70	" " " " " "
Third specimen lost	2.88	" " " " " "
Average	3.46	" " " " " "

In the abrasion tests, individual results show considerable variation from the average, as is apparent from the results of testing three specimens made of the same materials at different dates, given in Table II. The maximum variation from the average is 16.8 per cent. While this variation may seem excessive, it should be kept in mind that the chances for variation with a gravel specimen are larger than with crushed stone. This is due to the fact that a different number and size of the stones are knocked out of the surface of the gravel concrete in each of the specimens, while concrete made of ordinary crushed stone shows no such loss. Fig. 6 shows four of the abrasion test specimens after 2000 revolutions.

RESULTS OF TESTS.

The results of the various tests have been arranged in Table III, according to resistance of the concrete to abrasion.


2952A
2783
2869
2922 A
A

Limestone. Gravel. Slag. Gravel.

FIG. 6.—Specimens after Test.

TABLE III.—SHOWING THE COMPRESSIVE AND TENSILE STRENGTH AND RESISTANCE TO ABRASION OF CONCRETE MADE FROM VARIOUS AGGREGATES.

EXCEPT WHERE NOTED, WELL-GRADED COLORADO RIVER SAND (COMPOSED OF SHARP GRAINS OF FLINT, QUARTZ, AND LIMESTONE) WAS USED AS THE FINER AGGREGATE.

Laboratory No.	Nature of Aggregate.	Size of Aggregate, in.	Proportions.	Grading of Aggregate, Percentage Passing Sieve:					Results of Concrete Tests.			Appearance of Abrasion Specimen after 2000 Revolutions.
				2 in.	$1\frac{1}{2}$ in.	1 in.	$\frac{1}{2}$ in.	$\frac{1}{4}$ in.	Compressive Strength, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Loss in 1000 Revolutions, per cent.	
	GRAVEL.											
2783[a]	Composed of rounded and some flat pebbles of limestone and sandstone.	$\frac{1}{4}$-2	1 : 2 : 4	100	95	78	29	0	3290	247	2.6	Specimen contained a number of swellings and depressions and a few stones were knocked out of surface; however, specimen was fairly uniform. Swellings due to the large size of the aggregate.
2765	Composed of clean, smooth pebbles of quartz, flint, and a little limestone.	$\frac{1}{4}$-2	1 : 2 : 4	100	86	50	[illegible]5	2	2860	222	2.8	Surface very rough. Flints worn practically not at all, and a number were knocked out of surface. The limestones wore equally with the mortar.
2922	Composed of clean, smooth pebbles of flint, quartz, and gneiss.	$\frac{1}{4}$-2	1 : 2 : 4	100	87	71	7	0	2010	284	3.3	Surface wore very rough, the softer stones wearing equally with the mortar, while the hard flints and quartzites were worn very little.
2821	Composed of pebbles of granite, flint, quartz, and limestone.	$\frac{1}{4}$-$1\frac{1}{2}$	1 : 2 : 4	100	100	71	39	3	2720	260	4.1	Surface quite rough, the flints and quartzite wearing practically not at all, while limestones wore equally with the mortar.
2872	Composed of rounded pebbles of limestone, sandstone and shells.	$\frac{1}{4}$-$1\frac{1}{4}$	1 : 2 : 4	100	100	91	43	0	2180	244	4.5	Surface uniform and smooth, similar to limestone specimens.

[a] Sand from same pit as gravel screened below $\frac{1}{4}$ in. having 23 per cent retained on $\frac{1}{8}$-in. sieve, 35 per cent retained on No. 10 sieve, 94 per cent retained on No. 48 sieve, 100 per cent retained on No. 200 sieve.

TABLE III.—SHOWING THE COMPRESSIVE AND TENSILE STRENGTH AND RESISTANCE TO ABRASION OF CONCRETE MADE FROM VARIOUS AGGREGATES (*Continued*).

EXCEPT WHERE NOTED, WELL-GRADED COLORADO RIVER SAND (COMPOSED OF SHARP GRAINS OF FLINT, QUARTZ, AND LIMESTONE) WAS USED AS THE FINER AGGREGATE.

Laboratory No.	Nature of Aggregate.	Size of Aggregate, in.	Proportions.	Grading of Aggregate, Percentage Passing Sieve:					Results of Concrete Tests.			Appearance of Abrasion Specimen after 2000 Revolutions.
				2 in.	$1\frac{1}{2}$ in.	1 in.	$\frac{1}{2}$ in.	$\frac{1}{4}$ in.	Compressive Strength, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Loss in 1000 Revolutions, per cent.	
	GRAVEL (*Continued*).											
2930-A	Rounded pebbles of limestone with some flint and quartz.	$\frac{1}{8}$-2	1 : 2 : 4	100	92	75	37	7	2950	...	4.6	Surface rather rough, the limestones wearing equally with the mortar, while flints and quartz protrude as much as ½ in. from surface. Some stones have been knocked out of the specimen.
2732	Composed of rounded pebbles of flint, quartz, granites, and limestone.	$\frac{1}{4}$-2	1 : 2 : 4	100	84	62	21	0	3100	268	4.7	Surface very rough; flints fail to wear to any extent, granites crack and are smooth, while limestone wears equally with the mortar.
2930-D[1]	Composed of limestone pebbles with some flint and quartz.	$\frac{1}{8}$-2	1 : 2.2 : 4	100	92	75	37	7	1580	...	7.9	Surface rather rough, the limestone pebbles wearing equally with the mortar, while the flints have worn practically not at all.
	LIMESTONE.											
2795	Rather soft, with a sharp angular fracture.	$\frac{1}{4}$-$1\frac{1}{2}$	1 : 2 : 4	100	100	76	24	2	3265	284	3.0	Surface rather uniform. Where soft stones were not surrounded by harder ones, a slight depression developed.
2929	Fairly hard and uniform, with a rough fracture.	$\frac{1}{4}$-$1\frac{1}{2}$	1 : 2 : 4	100	99	70	14	2	2670	222	3.0	Surface very uniform. Some slight depressions and swellings were apparent, due probably to some inequalities in the hardness of the stone.

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[1] Sand from same pit as gravel screened below ¼ in. having 4 per cent retained on No. 10 sieve, 63 per cent retained on No. 48 sieve, and 95 per cent retained on No. 200 sieve.

TABLE III.—SHOWING THE COMPRESSIVE AND TENSILE STRENGTH AND RESISTANCE TO ABRASION OF CONCRETE MADE FROM VARIOUS AGGREGATES (*Continued*).

EXCEPT WHERE NOTED, WELL-GRADED COLORADO RIVER SAND (COMPOSED OF SHARP GRAINS OF FLINT, QUARTZ, AND LIMESTONE) WAS USED AS THE FINER AGGREGATE.

Laboratory No.	Nature of Aggregate.	Size of Aggregate, in.	Proportions.	Grading of Aggregate, Percentage Passing Sieve:					Results of Concrete Tests.			Appearance of Abrasion Specimen after 2000 Revolutions.
				2 in.	$1\frac{1}{2}$ in.	1 in.	$\frac{1}{2}$ in.	$\frac{1}{4}$ in.	Compressive Strength, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Loss in 1000 Revolutions, per cent.	
	LIMESTONE (*Continued*).											
2755	Rather soft, some very soft stones, occasional cherts.	$\frac{1}{4}$-$1\frac{1}{4}$	1 : 2 : 4	100	100	84	18	1	2600	236	3.6	Surface smooth, except an occasional chert, which produces a swelling on the surface.
2831	Angular fragments, a few of which are soft stones.	$\frac{1}{4}$-1	1 : 2 : 4	100	100	98	20	1	2120	230	3.7	Surface in good condition, being smooth and free from depressions.
2906-B[1]	Flat and angular fragments of limestone, rather uniform.	$\frac{1}{8}$-$1\frac{1}{4}$	1 : 2 : 4	100	100	94	42	14	2160	202	3.8	Surface in good condition, being quite smooth.
2830	Angular fragments, but contains some soft stone.	$\frac{1}{4}$-2	1 : 2 : 4	97	84	48	10	3	2460	228	4.0	Surface somewhat rough; the larger stones did not wear equally with the smaller ones and the mortar.
2810	Somewhat soft, but uniform.	$\frac{1}{8}$-$1\frac{1}{2}$	1 : 2 : 4	100	100	65	33	9	....	...	4.0	Surface rather smooth; however, there were a few slight swellings and depressions.
2906	Flat and angular fragments, but uniform.	$\frac{1}{8}$-$1\frac{1}{4}$	1 : 2 : 4	100	100	94	42	14	2120	260	4.4	Surface uniform and smooth.
2952[1]	Somewhat soft, flat and angular fragments.	$\frac{1}{8}$-1	1 : 2 : 4	100	100	100	51	13	1785	232	4.8	A superior surface, being uniform and smooth.
	TRAP ROCK.											
2569-I	Hard and tough, angular fragments.	$\frac{1}{4}$-$1\frac{1}{4}$	1 : $1\frac{1}{2}$: 3	100	99	92	23	0	2580	216	1.8	Surface somewhat rough, the stones protruding; the mortar was worn considerably.

[1] Well-graded Colorado River sand under ¼ in.

TABLE III.—SHOWING THE COMPRESSIVE AND TENSILE STRENGTH AND RESISTANCE TO ABRASION OF CONCRETE MADE FROM VARIOUS AGGREGATES (*Continued*).

EXCEPT WHERE NOTED, WELL-GRADED COLORADO RIVER SAND (COMPOSED OF SHARP GRAINS OF FLINT, QUARTZ, AND LIMESTONE) WAS USED AS THE FINER AGGREGATE.

Laboratory No.	Nature of Aggregate.	Size of Aggregate, in.	Proportions.	Grading of Aggregate, Percentage Passing Sieve:					Results of Concrete Tests.			Appearance of Abrasion Specimen after 2000 Revolutions.
				2 in.	$1\frac{1}{2}$ in.	1 in.	$\frac{1}{2}$ in.	$\frac{1}{4}$ in.	Compressive Strength lb. per sq. in.	Tensile Strength, lb. per sq. in.	Loss in 1000 Revolutions, per cent.	
	TRAP ROCK (*Continued*).											
2871-D[1]	Hard and tough, angular fragments.	$\frac{1}{8}$-1	1 : $1\frac{1}{2}$: 3	100	100	100	57	14	3490	312	2.1	A superior surface, free from depressions or any inequalities.
2569-H	Hard and tough, angular fragments.	$\frac{1}{4}$-$1\frac{1}{4}$	1 : 2 : 4	100	99	92	33	0	....	288	2.1	Surface comparatively rough; stones wore round, while mortar was worn considerably, leaving the stones protruding. The actual wear, however, was uniform.
2 871 B[1]	In angular fragments. Very hard and tough.	$\frac{1}{8}$-1	1 : 2.2 : 4.4	100	100	100	57	14	1970	248	2.9	Surface comparitively smootn, except for an occasional large stone which protrudes above the surrounding surface.
2871-A[1]	Hard and tough, angular fragments.	$\frac{1}{8}$-1	1 : 2 : 4	100	100	100	57	14	2300	280	3.2	Surface comparatively uniform, presenting an even surface. Some of the larger stones protruded slightly above surrounding surface.
	SLAG.											
2889	Sharp, angular fragments, the greater proportion dense; remainder somewhat porous.	$\frac{1}{4}$-2	1 : 2 4	100	79	62	8	1	2850	...	2.4	Surface rather uniform, although a number of the larger stones failed to wear equally in amount to the smaller ones, causing slight swellings. Surface rather porous and might be a good surface to treat with a bituminous material.

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[1] Used a medium fine sand having 0.0 per cent retained on No. 10 sieve, 65 per cent retained on No. 48 sieve, and 98 per cent retained on No. 200 sieve. Composed of flint and quartz grains.

These results are accompanied by brief remarks upon the appearance of the specimen after 2000 revolutions. While the number of tests is somewhat limited for judging the relationship existing between the various tests, it seems that there is practically no such relation.

Abrasion Tests.—If the relation between the number of revolutions of the specimen and the traffic were known, it would be possible from the test to determine how long a concrete road could be expected to be satisfactory. It does not seem unreasonable to suppose that the effect of one revolution of the specimen is equivalent to that of ten steel-tired vehicles. Certainly the tests show this in a relative sense, which is quite satisfactory.

One of the most important factors in road construction is to procure a surface that is uniform and will wear uniformly. Especially is this true with a rigid road such as concrete makes. It should be smooth enough to allow the vehicles to roll over the surface without appreciable impact, and yet rough enough to afford a horse good footing, especially in wet weather and upon steep grades. It is, therefore, just as important to know the character of the surface of the concrete after the test as it is to know the numerical loss.

The Gravel Aggregate.—The surface of most of the gravel concrete specimens was rough. Such gravel specimens as No. 2930-A and No. 2732 (Fig. 7), containing pebbles of varying hardness, produce a surface in which the hard flints wear but little, while the softer limestones wear equally with the mortar. In many cases these flints protrude as much as ½ in. above the surrounding surface. Other gravels, such as No. 2765, are composed of pebbles of equal hardness. However, they are so much harder than the surrounding mortar that the latter wears much faster than the stones, causing a somewhat uniform, but extremely rough, surface. The hard stones are knocked out, causing more than normal pitting and loss in the specimen. An exception to the general run of the gravels is one such as No. 2872, composed of limestone and sandstone pebbles only, which have approximately the same hardness. In this case the pebbles themselves were but little, if any, harder than the mortar, and consequently the specimen wore surprisingly uniformly. In this particular instance, it should be noted that the gravel contained no pebbles over $1\frac{1}{4}$ in. in size, which undoubtedly influenced the satisfactory condition of the surface.

Crushed Stone Aggregates.—Where crushed limestone was used as the coarse aggregate, the surface as a rule was smooth and uniform. The limestones tested were all comparatively soft and they wore equally with the mortar. In specimens where the aggregate contained some large stones (No. 2830) the surface became somewhat wavy, due to the fact that the larger stones did not wear as fast as the smaller ones. In those specimens in which the aggregate was below $1\frac{1}{2}$ in. in size, the surfaces were very satisfactory (Fig. 8). In one or two of the specimens that contained some very soft stones, there was a tendency for these stones to be pitted. This, however, does not seem to be any serious disadvantage.

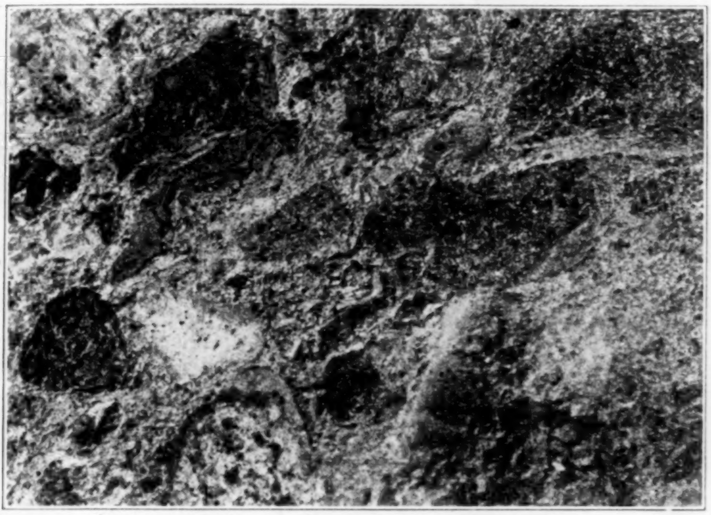

FIG. 7.—Gravel Specimen (about $\frac{6}{10}$ Natural Size). Surface showing Wear on Limestone Equal to Mortar, while Flint Protrudes; also One Stone Knocked Out.

With the crushed trap-rock specimens the results are interesting. The stone is a basalt having high resistance to abrasion in the Deval Machine, and very tough. Trap rock from the same quarry was used in all the tests. In the 1 : 2 : 4 concrete specimens, the mortar wore faster than the stone, giving the surface a distinctly mosaic appearance. This was more noticeable with the larger-sized aggregates.

In the 1 : $1\frac{1}{2}$: 3 concrete specimens, the mosaic appearance of the surface was still apparent with the larger-size aggregate; but with that graded only to 1 in. the surface was very satisfactory, having but few protruding stones. This latter specimen also had a high resistance to abrasion. (Fig. 9.)

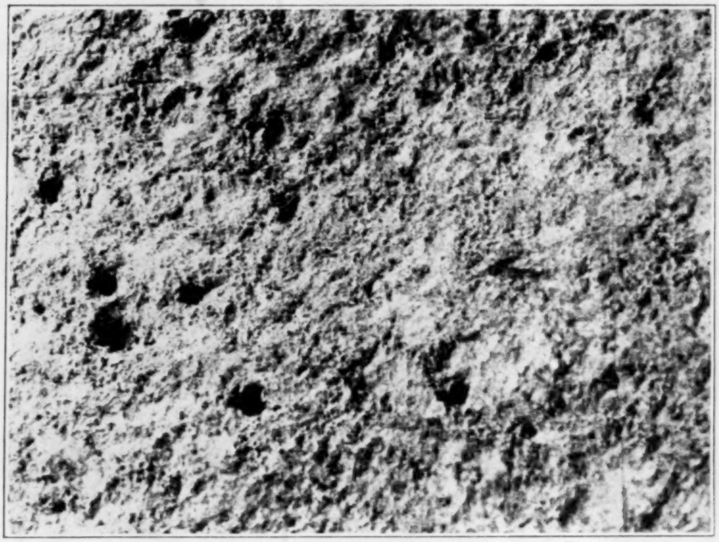

FIG. 8.—Limestone Specimen (Natural Size). Surface showing Uniform Wear. Dark Spots are Air Cavities in Concrete.

The specimen in which slag was used for the coarse aggregate (No. 2889) had a rather high resistance to abrasion and a fairly uniform surface. It is believed that if this aggregate was limited in size to about $1\frac{1}{4}$ in. the surface would be very smooth. The surface appeared rather porous when enough of the slag fragments had been worn away to expose their interior. From this apparent porosity of the specimen, it would seem that it should make a satisfactory surface for a treatment of asphalt or tar.

Tension Tests.—From the results given in Table III it is apparent that no relation exists between the compressive and tensile strengths of any given concrete. When the limestone concrete fails in tension practically all of the stones are broken, but with gravel there are always quite a number of stones that are pulled out rather than broken. The number broken depends upon the smoothness of the stones, but even with the very smooth stones a good proportion of them break.

It is preferable that the tensile strength of concrete for road construction shall be obtained by using superior aggregates rather than additional cement, because the contraction due to setting of the concrete increases with the amount of cement, which tends to nullify the extra strength obtained.

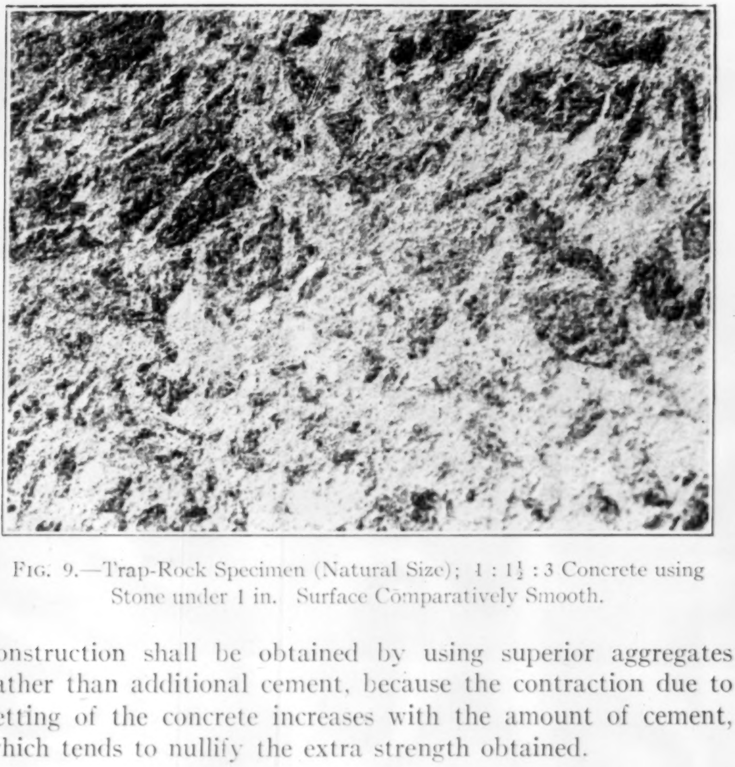

FIG. 9.—Trap-Rock Specimen (Natural Size); 1 : 1½ : 3 Concrete using Stone under 1 in. Surface Comparatively Smooth.

COMPARISON OF FIELD AND LABORATORY CONCRETE.

There is a great similarity between the results obtained in the laboratory and those shown in actual construction. A portion of San Pedro Avenue in San Antonio, Texas (Fig. 10), was constructed about four years ago of a concrete composed of 1 part of cement, 2 parts of a medium fine sand, and 4 parts of a soft local limestone. This street carries a fairly heavy traffic and is in excellent condition to-day, being smooth and free from depressions. While this is a poorer concrete than any of those tested, the appearance of the surface is practically the same as that of the surface of the limestone specimens. The surface of this street would seem to indicate that when a soft stone is used, the mortar also should be weak so that both should wear equally. An idea of the surface of the street can be gotten from Fig. 10.

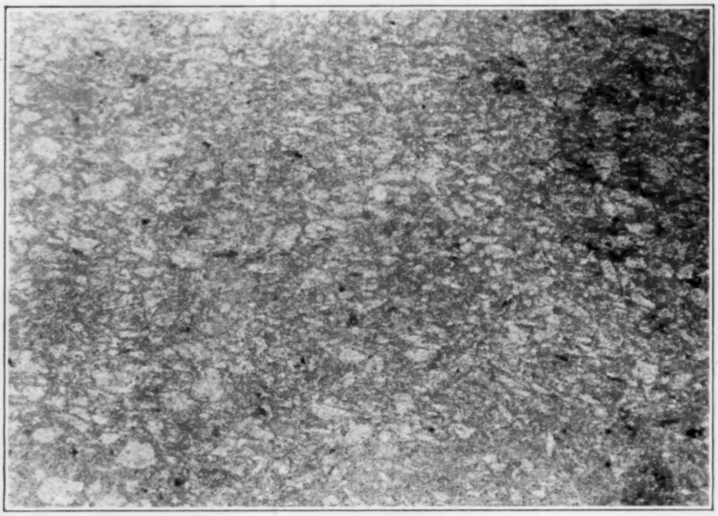

FIG. 10.—San Pedro Ave., San Antonio, Texas. Surface of Street showing Uniform Wear of Limestone Aggregate.

On North Laredo Street, in the same city (Fig. 11), a trap-rock concrete has been in use for something less than two years. The proportion of the concrete was 1 : 2 : 4, the stone being the same trap rock as that used in the laboratory tests, except that it was graded from $\frac{1}{4}$ in. to 2 in. The condition of the surface of the street, however, brings out the similarity of the wear with that of the laboratory specimens. The mortar is wearing rapidly away from the stones, leaving them protruding from the surface. The large size stone used aided this result considerably, it is believed. Fig. 11 shows a small portion of this surface.

A crossing for pedestrians on a fairly heavy traffic street in Austin, Texas, is illustrated in Fig. 12. This is a gravel concrete in which the flints protrude while the limestone pebbles have worn equally with the mortar. The gravel in the section was the same as that in specimen No. 2821, but the proportion of the mortar is not known to the writer.

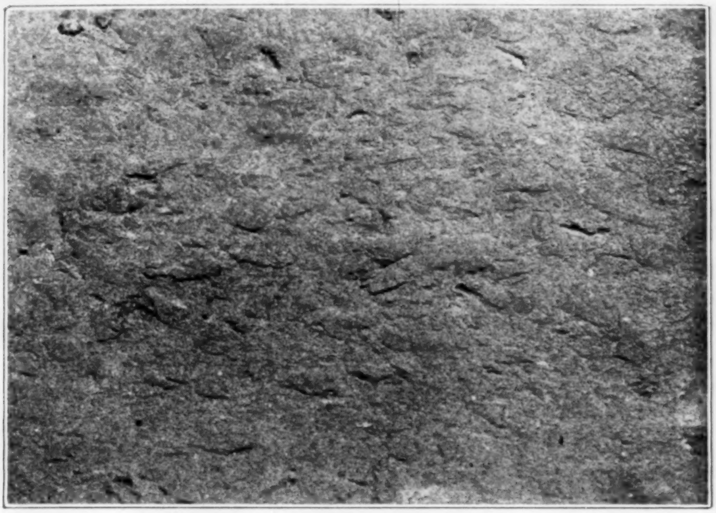

FIG. 11.—North Laredo St., San Antonio, Texas. Surface of Street showing Trap-Rock Concrete in which the Mortar is Wearing Faster than the Coarse Aggregate.

While the above are the only examples at hand illustrating the actual wear of concrete roads, the writer has been informed of many others which bring out the various points shown by the laboratory tests.

CONCLUSION.

The two most important essentials for a satisfactory road are, first, uniformity of wear; second, a minimum of wear. Whenever these two are combined with a high tensile strength, the most satisfactory concrete road is found.

The conclusions drawn from these tests are as follows:

1. The uniformity of wear is obtained when the mortar and the coarse aggregate wear equally, such as when crushed limestone or limestone gravel is used in a 1 : 2 : 4 mix.

2. The coarse aggregate should be limited in size to about $1\frac{1}{2}$ in.

3. When hard, tough stone is used, the size should be limited to about 1 in. and the cement content increased.

4. It is questionable if a richer mix than a 1 : 2 : 4 is an economical one to use with crushed limestone of the ordinary hardness. Crushed slag when hard and uniform should be satisfactory as a concrete road aggregate from the standpoint of wear.

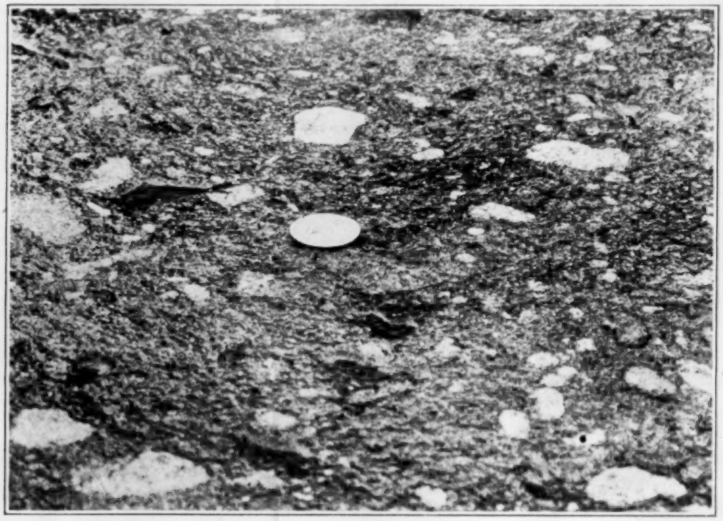

FIG. 12.—Concrete Street Surface, Austin, Texas, showing Character of Gravel Surface with the Flints Protruding while the Limestones have Worn Equally with the Mortar.

5. In a 1 : 2 : 4 concrete, a gravel composed of very hard stones such as flint, or quartz, does not wear uniformly.

6. The action of the cubical shot on the test specimens is a trifle more severe than the traffic on the road.

7. It cannot be said that either the crushed stone or gravel tested is superior as an aggregate to produce concrete having a higher tensile strength.

DISCUSSION.

Mr. Mattimore.

MR. H. S. MATTIMORE.—Mr. Nash deserves credit for presenting this paper at a time when so many concrete roads are being constructed with various kinds of aggregates. In order to use aggregates intelligently in concrete highways, it is essential to have some method of test to accelerate wear so that the more desirable aggregates may be determined in advance. The one criticism regarding the method of test described by Mr. Nash is that the form of test specimen used is not susceptible to various surface finishes which are now being given great attention in the construction of concrete highways.

I do not agree with the author's conclusion in regard to limiting the size of coarse aggregate to $1\frac{1}{2}$ in. This was the first specification used in the construction of this type of highway, having been recommended by the American Concrete Institute. Within the past several years the practice has been to increase the size of the coarse aggregate, and the Concrete Institute and the National Conference on concrete road building have now recommended a maximum size of 2 in. instead of $1\frac{1}{2}$ in.

We have been making tests along this line in the New York State laboratory. Our method includes an abrasion test and a somewhat severe impact test. This latter we have concluded is more reliable than the abrasion test from the standpoint of accelerating the wear. In this test we have found that the larger size aggregates, up to 2 or even $2\frac{1}{2}$ in., wear more uniformly than the smaller sizes. Also, they are less likely to be knocked out of the matrix.

Regarding the comparison of gravel and crushed stone, I believe the crushed stone is better, as it is more likely to be uniform in quality than the gravel and its shape tends to give it a better bond with the matrix.

As to gravel sizes, I am well satisfied that the larger size gravels are more satisfactory than the smaller. Inspection of highways in which gravel has been used as coarse aggregate indicates that the small sizes "kick out" and tend to produce spalling at the joints, especially under horse traffic. The large

Mr. Mattimore. sizes, from 1 to $2\frac{1}{2}$ in., seem to wear more uniformly and to have a better bond.

Mr. Abrams. MR. D. A. ABRAMS.—We have been investigating this very important subject of the wear of concrete. At the meeting a year ago I presented a brief discussion on our method of making the tests, without giving any specific results, since prior to that time the work had been confined almost entirely to a development of the method itself.[1] Our method deviates somewhat from that described by Mr. Nash. The machine and the procedure are similar in some respects. Instead of using the ring of concrete, however, we use a series of rectangular blocks placed to form a ring inside of which the charge of shot is placed. It strikes me that the method of testing which we have used has many advantages. The wear on the concrete comes on the top surface of the block in exactly the same position in which the concrete is finished. That is one advantage. Another advantage is that blocks may be cut out from a road or pavement or floor which has been in service, placed in the machine, and tested in exactly the same way as the specimen made specially for testing. Still another advantage is that the smaller size of test piece is more easily made and handled and I believe will give just as uniform results, and permit of a larger variety of tests than is possible by using the ring.

It seems to me that from the standpoint of testing materials, it is not necessary, as Mr. Nash has done, to make so many different kinds of tests. By that I refer particularly to comparisons between the briquettes that he has devised and the compression cylinder. I take it that the only purpose of these tests is to procure a measure of the quality of the concrete used. If we grant that either of these specimens would give the information desired, it would certainly seem that the other should be eliminated. In view of the fact that the stress in a briquette is not uniformly distributed across the section, I do not believe that this is a scientific form of test piece. A briquette of this form is an excellent example of a member eccentrically loaded. This subject has been discussed very thoroughly in the meetings of the sub-committees of Committee C-1 on Cement which have

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[1] "A Method of Making Wear Tests of Concrete," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 194 (1916).

Mr. Abrams.

to do with the tests of cement. It is largely due to this consideration that a recommendation has been adopted concerning a standard compression test of cement mortar. We observe in tests of this kind that certain results are obtained from a briquette that are not at all in accord with what we find in the concrete itself. The most noteworthy of these is the fact that in the briquette test the strength of the briquette invariably falls off after a certain period of time, ranging from 28 days up to 3 or 6 months. The same cement used in concrete does not show this retrogression in strength. Analyses made by two different writers[1] have brought out the reason for this quite clearly. The peculiarities of the briquette results can be attributed entirely to the non-uniformity of stress across the section. Due to this cause the speaker is fully convinced that the briquette test gives entirely erroneous results in many instances. In view of this consideration it seems that a compression test can be expected to give us all the information we can hope to get from any number of specimens of different form.

Mr. Goldbeck.

MR. A. T. GOLDBECK (*by letter*).—The longevity of concrete roads is subject to much discussion at the present time. The design of concrete roads is not yet on a theoretical basis, the best method of construction has not as yet been developed, and finally, the most suitable materials and their combinations are still being investigated. It is pointed out by Mr. Nash that concrete roads crack, due to a number of causes, such as shrinkage resisted by friction, unequal settlement with the production of excessive bending stresses, etc. Cracks imply excessive tensile stress, and therefore a study of the tensile strength of concrete, such as made in Mr. Nash's paper, should prove of great benefit in the future, when the theoretical design of concrete roads shall have been placed on a sound basis. Again, as Mr. Nash points out, concrete roads wear, and the rapidity with which they wear is dependent on a number of factors, such as proportions, kind of aggregate; and to these may be added, consistency of mixture, method of laying, method of mixing, time of curing, and several other factors.

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[1] See J. B. Johnson, "Materials of Construction," 4th edition, p. 435; also *Proceedings*, Inter. Assoc. Test. Mats., 1912, Second Section; paper 58.

Mr. Goldbeck. The study of the wear of concrete roads by means of laboratory tests is one greatly to be commended, since a suitable test of this kind may in a short time give information which could be obtained from the observation of roads only after an extended period. Moreover, by means of such a test, the relative suitability of a number of available sources of supply may be determined before construction of the road is begun, and thus the materials best suited for producing a long-lived road will have been secured.

The testing of concrete in tension is a matter of no little difficulty, and a number of forms of specimens and apparatus have been designed. It would seem that Mr. Nash has developed an admirable type of tension clip for testing his briquette-shaped concrete specimens, and the uniformity of results obtained is an excellent proof of the efficiency of his apparatus. It has been observed by the writer, as well as by a number of others interested in concrete roads, that limestone concrete generally has fewer cracks than gravel concrete, or that made from other aggregates. A very comprehensive study of concretes in tension, made with various kinds of coarse aggregates, and tested when only 7 days old, as well as at later periods, would be very enlightening, and should explain this phenomena. It is a well-known fact that great shrinkage takes place in concrete roads as soon as the earth-protective covering is removed, permitting the concrete to dry out. That concrete which is strongest in tension at early periods should theoretically possess the fewest number of cracks. Tests such as above outlined would show which aggregate makes the strongest concrete in tension at early periods. Some preliminary results obtained by the writer at the Office of Public Roads and Rural Engineering on modulus of rupture of concretes containing different coarse aggregates indicate that limestone concrete at 7 days is stronger than gravel, trap rock, or gneiss, all of the coarse aggregates having the same mechanical analysis.

The abrasion test used by Mr. Nash, as well as by Professor Shoop of the University of Minnesota, is capable of giving very valuable results on the wear-resisting qualities of different kinds of concrete, as evidenced by the table in Mr. Nash's paper. The results obtained agree well with those of observation of concrete roads, and the tests should be very suitable for making a preliminary study of materials. An abrasion test, however, can be made to serve a more useful purpose than that of preliminary studies of materials. There are many miles of concrete roads already laid, built of an enormous variety of aggregates mixed in various proportions and laid under various conditions of drainage and weather. Most of these roads are good and some of them are bad. Would it not be desirable to develop an abrasion test in which samples actually taken from these roads could be tested? In this way the known behavior of the road could be correlated with the behavior of part of the same road in a laboratory. Such a test is now being developed. **Mr. Goldbeck.**

In conclusion, the writer wishes to acknowledge his appreciation of the value of the results obtained by Mr. Nash. They should aid the attainment of that ideal of the concrete road engineer, the crackless and most wear-resisting concrete road.

Mr. Nash. MR. J. P. NASH (*Author's closure by letter*).—The point made by Mr. Abrams that the tension tests on concrete are unreliable is not supported by the results of the tests recorded in my paper. In fact, the results of the tension tests check with one another much closer than would compression tests made under similar conditions. Attention is called to Table I showing the results of tension tests of specimens made on different days. The maximum error from the average is 7.3 per cent, which is rather small when it is considered that the specimens were all made under different conditions. Should there be any secondary stresses, such as torsional or bending stresses, they must necessarily be small in amount, otherwise much larger errors would be shown in the results. With the apparatus used in making these tests, the secondary stresses were reduced to a minimum and it is questionable if there were any at all.

Mr. Mattimore speaks from a practical standpoint and his opinion should carry great weight. As has been mentioned in my paper, the action of the cubical shot tends to dig into the mortar to a greater extent than the actual impact from traffic on a road, but there is considerable doubt in my mind regarding the advisability of using trap rock graded up to $2\frac{1}{2}$ in. in size, which Mr. Mattimore says will produce a satisfactory surface.

COMPARISON OF HEAT-INSULATING PROPERTIES OF MATERIALS USED IN FIRE-RESISTIVE CONSTRUCTION.[1]

By W. A. Hull.

SUMMARY.

This comparison of materials themselves was preliminary to investigation of the fire-resistive properties of structural units.

Solid cylindrical specimens, 8 by 16 in., were given the same heat exposure, in a special gas-fired furnace. Temperatures were measured at four depths in the specimens, by means of thermo-couples. Burned clays, concretes, gypsums and lime mortar were included.

Rise of temperature was the most rapid through two of the clays. A more porous clay heated through as slowly as most of the concretes.

Concretes which heated through most rapidly were from gravel and from boiler refuse from bituminous coal. Trap rock and blast-furnace concretes heated through more slowly, while the limestone concretes heated through most slowly of all, due in part at least to the thermal effect of dissociation of $CaCO_3$ in a thin layer next the surface.

Gypsum specimens showed long temperature lags at about 105° C., due to dehydration. All gypsum specimens heated through more slowly than clays and concretes. The densest gypsum mixtures showed slight thermal superiority over the more porous ones, presumably due to greater heat-absorbing capacity.

Lime mortar compared favorably, thermally, with the concretes.

All concretes were weak after test, gravel concretes especially so, due presumably to the high expansion of quartz, with

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[1] This paper is presented with the permission of the Director of the Bureau of Standards.

sudden volume increase at about 575° C. Limestone concretes retained somewhat more strength than the other concretes.

The gypsums and the lime mortar retained very little strength.

These results indicate the importance of porosity in the heat-insulating qualities of burned clays and indicate a slight superiority of limestone concrete and distinct inferiority of gravel concretes as compared with other concretes as fire-resistive material. Thermal superiority of dense over porous gypsums, over the range of mixtures represented, is indicated.

COMPARISON OF HEAT-INSULATING PROPERTIES OF MATERIALS USED IN FIRE-RESISTIVE CONSTRUCTION.

By W. A. Hull.

This investigation was preliminary to the investigation of the fire-resistive properties of full-size columns, etc., and was intended as a study of the insulating properties of the materials commonly used as protective coverings for steel. Many data are available as to the conductivity, specific heat and other thermal properties of such materials, particularly of clays and concretes. Some of the best information regarding these properties has been supplied by Prof. Charles L. Norton.[1] Satisfactory data on the conductivity of burned clays is plentiful. The laws of heat flow are such, however, that calculations intended to compare the insulating properties of materials, in which various physical and chemical changes would take place at temperatures to be reached in heat exposures under contemplation, would necessarily be exceedingly complex, even if the thermal effects of these various changes were known quantitatively, which is not the case.

Method of Testing.

The most promising method, as well as the simplest, seemed to be that of subjecting a number of specimens of the materials to the same heat treatment, measuring temperatures at definite points within the specimen while the test was in progress. For this purpose, all materials were made up into specimens of cylindrical form, 8 in. in diameter by 16 in. long, provided with four holes, $\frac{1}{4}$ in. in diameter, extending through the piece longitudinally, for thermocouples. A gas-fired furnace, shown in Fig. 1, was built for the purpose. It was long enough to take three 16-in. cylinders placed horizontally, end to end. The test specimen was in the middle with a cylinder of material of low thermal conductivity at each end. Cylinders were placed with their ends in contact and the joints were covered

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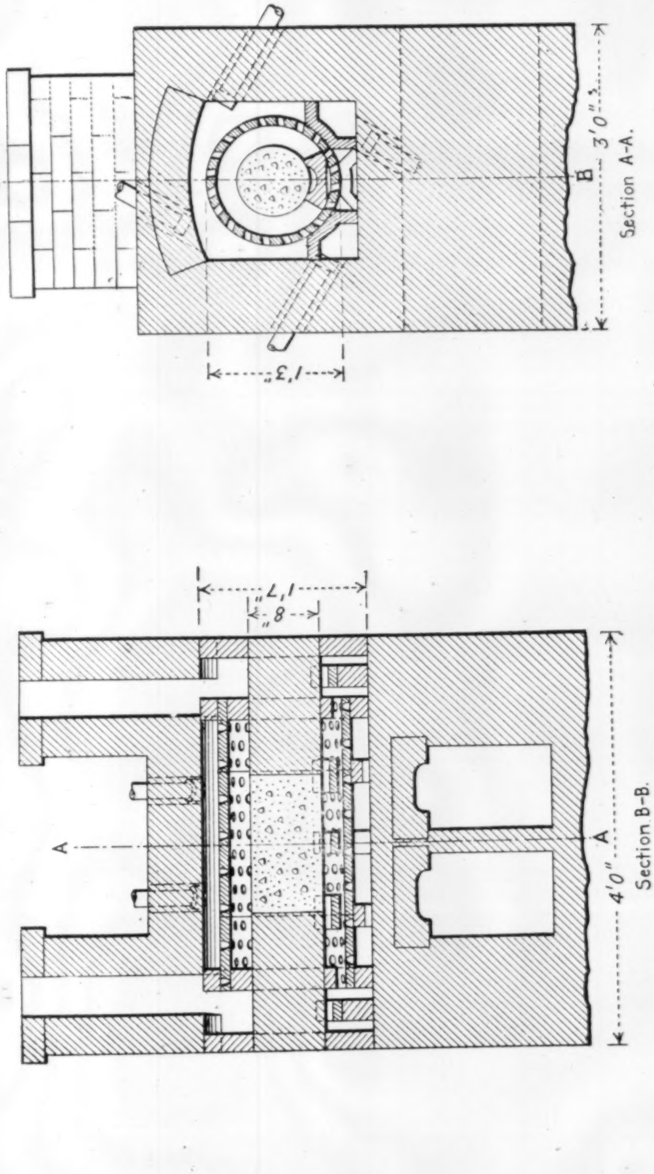
[1] "Some Thermal Properties of Concrete," *Journal*, Am. Soc. Mech. Engrs., June, 1913


B
B
3'0"
Section A-A.
1'3"
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8"
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A
4'0"
Section B-B.

FIG. 1.—Furnace for Testing Specimens.

with asbestos cement. This arrangement was intended to reduce end-flow of heat in the test specimen to a minimum.

The furnace was heated by four pairs of gas burners, so placed that their flames impinged obliquely on the furnace walls, crown and bottom, giving the furnace gases a tendency to flow annularly, without impinging on the specimen. In order to further equalize the heating of the specimen, both by convection and radiation, the specimen was surrounded by a perforated guard ring of fire clay. The furnace gases, having expended their first force in their annular travel around the outside of the guard ring, were drawn through the perforations with reduced velocity by a feeble stack draft. By this arrangement it was possible to give the specimen fairly satisfactory heat treatment.

Temperatures within the specimen were measured by means of thermocouples, at points $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$ and $3\frac{1}{2}$ in. from the cylindrical surface all in the middle of the specimen longitudinally. Furnace temperatures were measured above and below the middle of the specimen, between the specimen and the guard ring.

The furnace temperatures were raised at a nearly uniform rate to 927° C. (1700° F.), this temperature being attained in $1\frac{1}{2}$ hours, and held as nearly constant as possible for 2 hours, giving an exposure of $3\frac{1}{2}$ hours, at an average temperature of approximately 700° C.

Materials Used.—The materials included in the investigation were as follows:

1. Clays that are used in the manufacture of hollow-tile fireproofing;
2. Concretes, including two proportions of a number of aggregates;
3. Gypsums, including specimens from three manufacturers, prepared from mixtures differing in kind of filler and in the ratio of plaster to water;
4. One specimen of lime mortar;
5. One specimen of a new material.

In most cases, three specimens of each material or mixture were tested.

The clay for making specimens was furnished without charge by the National Fireproofing Co. from their Standard

Works at Perth Amboy, N. J., and by the Whitacre Fireproofing Co. from their Chicago and Waynesburg, Ohio, plants. Clay specimens were made and burned in the Pittsburgh Laboratories of the Bureau of Standards.

All concrete specimens were made up in the same laboratories. Universal Portland cement and Pittsburgh river sand of a grade used in concrete were used throughout the tests. Most of the aggregates were used in two proportions, 1 : 2 : 4 and 1 : 3 : 6. The gravel was the washed river gravel commonly used in concrete in Pittsburgh, but screened over a ½-in. screen after partial drying, to remove a variable quantity of fine material. These materials were purchased in open market. The gravel consisted almost entirely of sandstone pebbles.

The cinders were obtained from boilers using bituminous coal from the Pittsburgh seam.

The limestone was donated by T. K. Morris, Pittsburgh, Pa., and shipped from quarries at West Winfield, Pa. It was a high-calcium limestone.

The trap rock was from the vicinity of New York City.

Blast-furnace slag was donated by the Du Quesne Slag Products Co. It was crushed bank slag.

The concrete specimens were made by experienced men and approximately the same consistency was maintained, which was about that resulting from the use of 8 per cent of water with dry materials. The concrete was tamped in the molds. The cylinders were left 1 day in the molds and 6 days in a damp closet, after which they were kept in ordinary work room atmosphere, except three specimens which were stored in a damp closet until the day of testing, and three others which were stored in cans, with the atmosphere artificially kept at low humidity.

The gypsum specimens were supplied, ready made and without charge, by the United States Gypsum Co., the Keystone Plaster Co. and the American Cement Plaster Co. They were stored in atmospheric conditions until tested and given reasonable time for drying out.

The lime-mortar specimen was made in the laboratory. It was alternately wet and allowed to dry to promote setting, and finally seasoned for several months.

The new insulating material was received ready made and allowed to season thoroughly before test.

Results of Tests.

Figs. 2 to 8, inclusive, are given to show characteristic thermal behavior of classes of materials. Figs. 2 and 3 show contrast between the behavior of a dense and a porous specimen of burned clay. The difference can be accounted for largely by the lower thermal conductivity of the more porous specimen.[1]

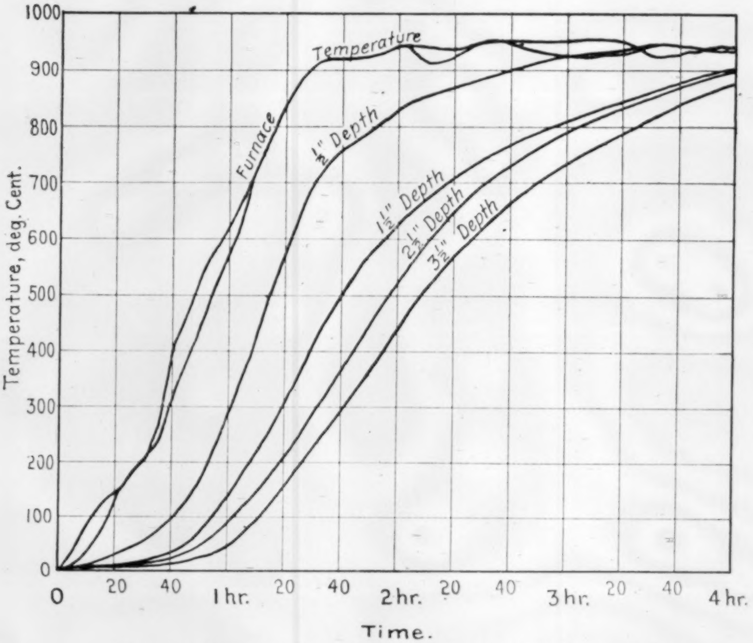

FIG. 2.—Fire Clay from Waynesburg, O.; Hard Burned.

The two concrete specimens showed a pronounced lag at about 100° C., which is attributable to absorption of heat by evaporation of water. Other changes taking place in the specimen at higher temperatures are not sufficient to give perceptible breaks in the curves. The one later break seen in each of the curves is considered a response to the break in the furnace curves.

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[1] Dougill, Hodsman and Cobb, "Conductivity of Refractory Materials," *Journal*, Soc. Chem. Ind., Vol. 34, p. 468 (1915).

A rather pronounced difference is to be noted in the rate of temperature progress in gravel concrete and limestone concrete, the limestone heating through more slowly than the gravel. (See Figs. 4 and 5.)

Figs. 6 and 7, illustrating the behavior of gypsum specimens, show that there is a comparatively long lag at about 107° C., at which gypsum dehydrates rapidly at atmospheric pressures. This indicates that the absorption of heat by the dehydration of gypsum is an exceedingly important factor in retarding heat flow through masses of gypsum. Contrast this with the curve shown in Fig. 8 for the second test of the specimen, which made about an average showing in the first test. Some allowance must be made for decreased size of the

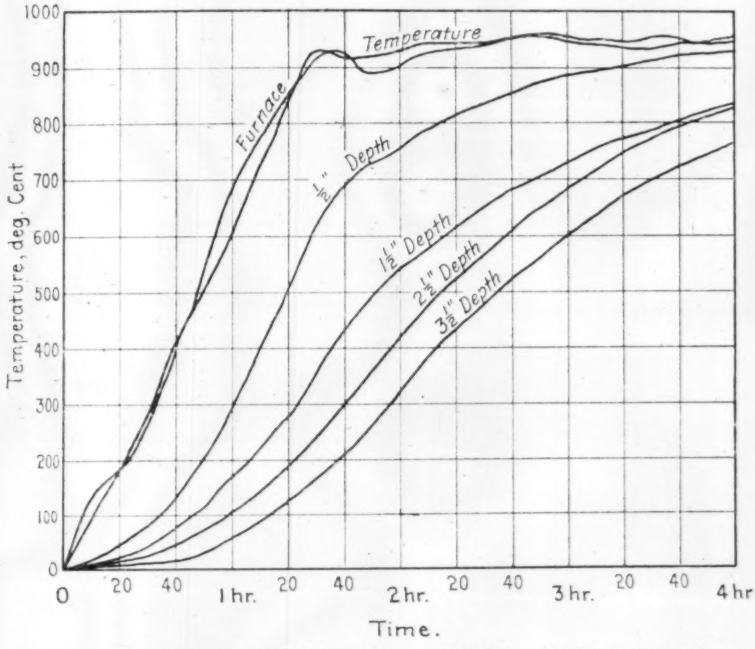

FIG. 3.—Plastic Surface Clay from near Chicago; Light Burned.

specimen due to shrinkage and cracks, but a great deal of the difference is thought to be due to the lack of the retarding influence of the dehydration.

The results of the tests have been collected and compared with the following in view:

1. To show the amount of protection afforded to relatively small steel reinforcement imbedded in a mass of the material. This is indicated by the time required for a temperature of 600° C. to be attained at depths of 1½ and 2½ in. A temperature of 600° C. is selected as the danger point for steel, based on a preliminary work by Dr. C. W. Kanolt,[1] in which it was shown that steel tubes, tested in a heated condition in compression, lost approximately 50 per cent of their strength at 635° C.

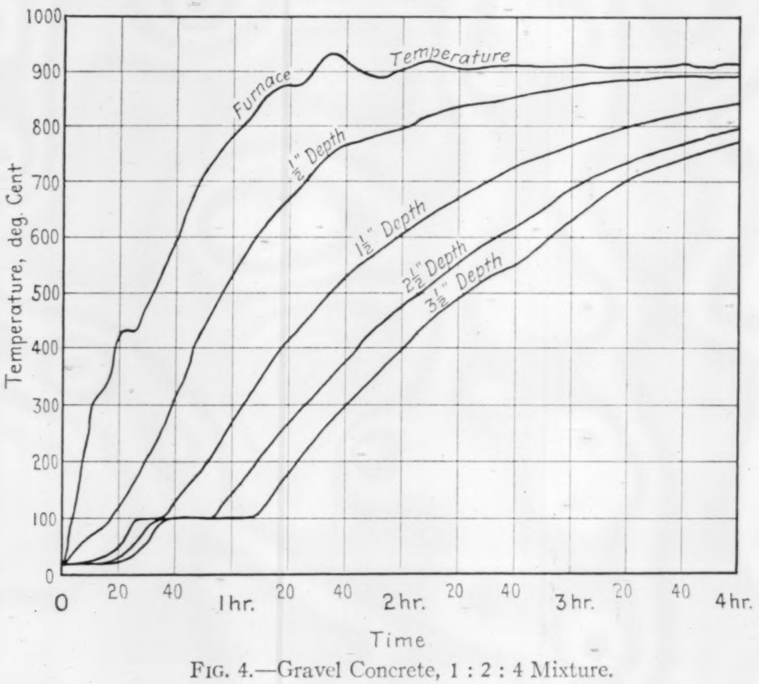

FIG. 4.—Gravel Concrete, 1 : 2 : 4 Mixture.

2. To show the amount of protection afforded to larger masses of steel, such as steel columns, and the amount of pro-

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[1] *Bulletin*, Bureau of Standards.

tection afforded the inner portion of a mass of concrete, such as a column, or of gypsum, such as a floor slab, by the outer portion of the same mass. This is indicated relatively by the temperature distribution through the specimen, at the end of 3½ hours.

Theoretical considerations indicate that the contrasts shown by these results would be greater in the case of actual, large steel members, protected with materials here compared; for it is found that it is the materials of low conductivity, in general, that show the lowest temperatures through the mass of the insulating material. If these materials show the lower temperatures in their own mass at a given time, it is reasonable to assume that the temperature of a steel member under their protection, would not only be at a lower temperature at that time than if under the protection of a material that had itself reached higher temperatures, but also that the steel would

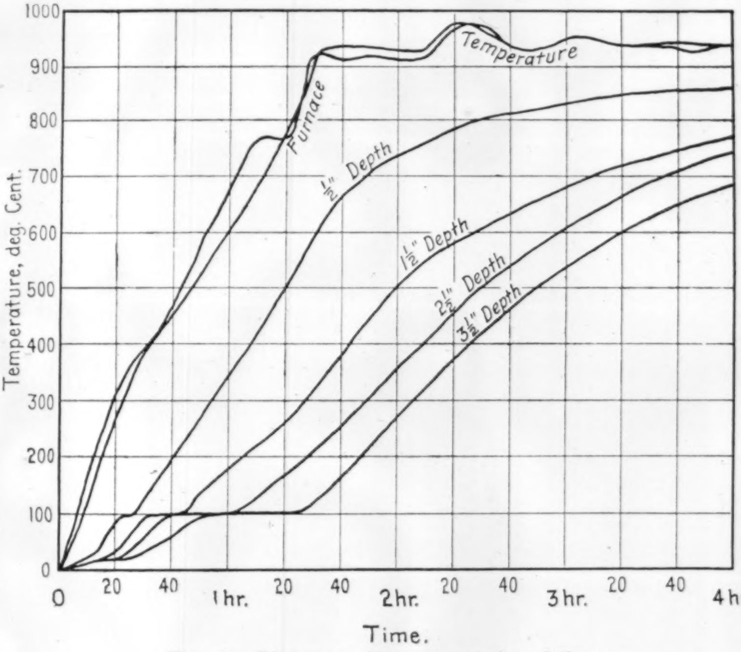


FIG. 5.—Limestone Concrete, 1 : 2 : 4 Mixture.

continue to receive heat more rapidly through the poorer insulator, until its temperature approached reasonably near to that of the external exposure temperature.

It is found that the length of time required to reach 600° C. at depths of $1\frac{1}{2}$ and $2\frac{1}{2}$ in. does not vary greatly in the clay and concrete specimens. This is not remarkable when it is considered that in materials of high conductivity, heat is more free to pass on to the interior, whereas, in materials of low conductivity it "banks-up" near the exposed surface. It is seen, however, that the denser clays, and the gravel and cinder concretes, make comparatively poor showings in this respect, and that the limestone concretes make a slightly better showing than the other concretes. The gypsums are found to be distinctly better than the clays and concretes in this respect. No comparison is possible among the gypsums in this connection,

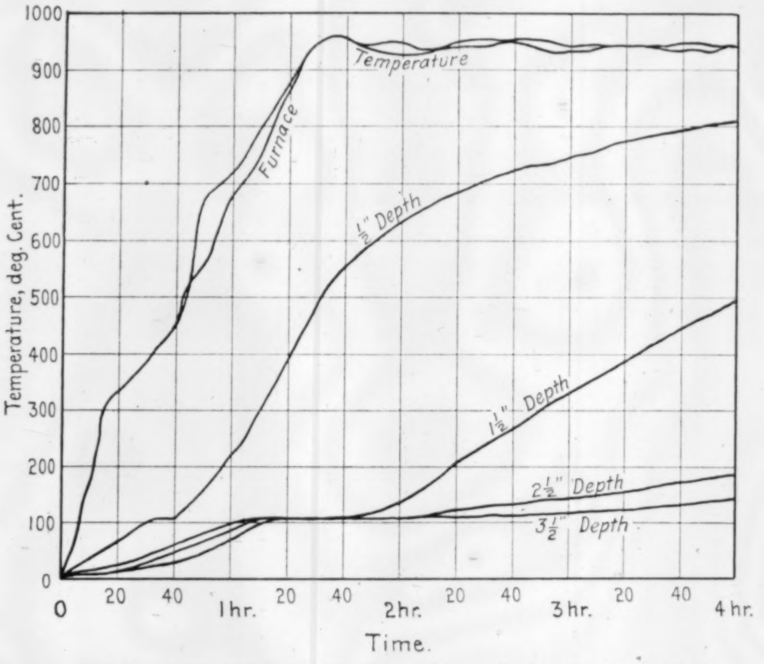


FIG. 6.—Nova Scotia Gypsum; 60 per cent Plaster, 40 per cent Water, No Filler.

because only one gypsum specimen in the entire lot reached 600° C. in 3½ hours.

In the matter of the final temperatures through the specimens, the denser clays attained the highest temperatures except those for the poorest cinder concrete. The more porous clays show final temperatures not much different from the average concretes.

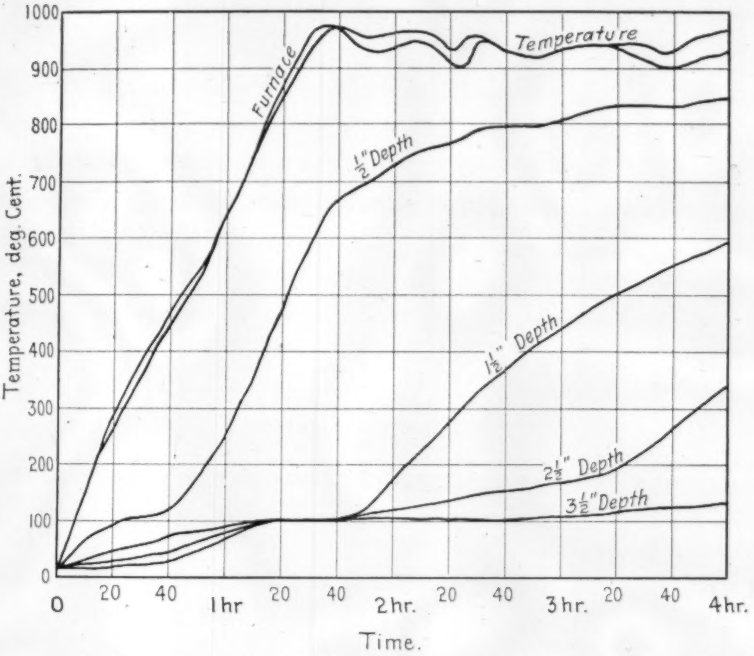


FIG. 7.—Nova Scotia Gypsum; 40 per cent Plaster, 60 per cent Water, No Filler.

Among the concretes, the gravel and the cinder concretes show the highest final temperatures and the limestone concretes the lowest. It should perhaps be pointed out that those concretes termed cinder concretes, had a considerable content of soft coke which presumably contributed to the rapid temperature rise by its own combustion. As for the limestones, the calcium carbonate in about $\frac{5}{16}$ in. of the thickness of the specimen, next to the cylindrical surface, was decarbonated in each case, which means that about 6 lb. of calcium carbonate were dissociated, giving a loss of weight of approximately 2½ lb., and requiring approximately as many heat units as the evaporation of 4.8 lb. of water. However, the effect of this heat absorption would not be as great as if it could have been evenly distributed throughout the mass, because heat units would be supplied more rapidly near the surface than farther from it.

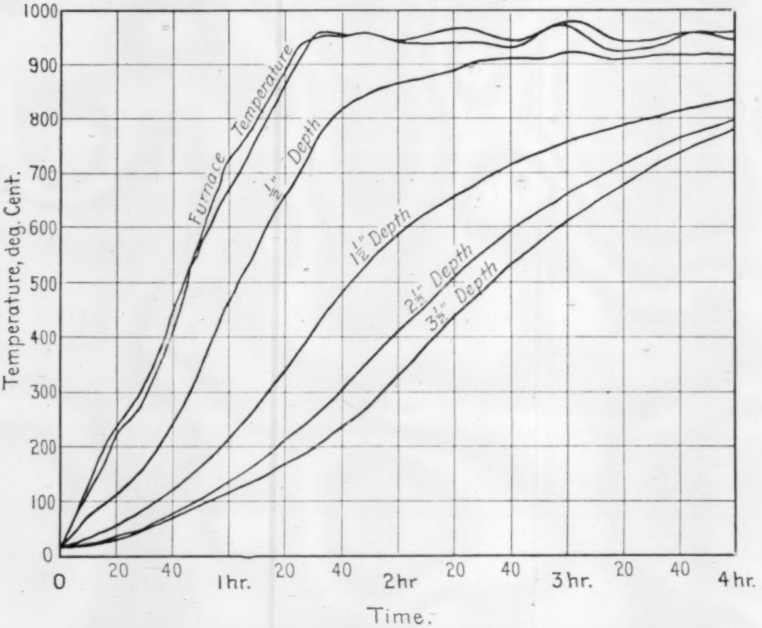
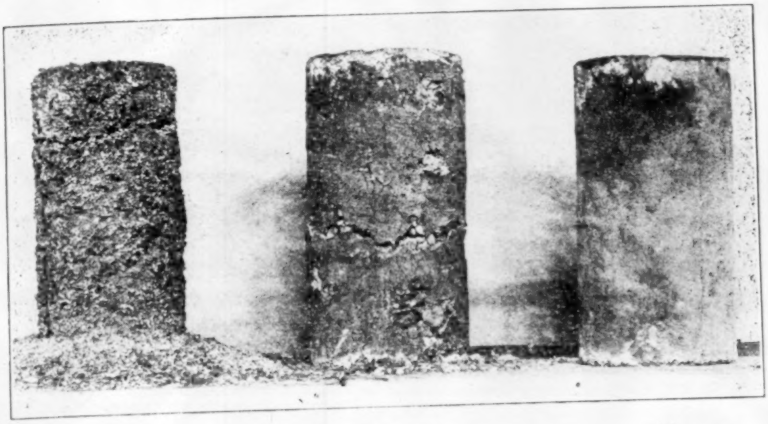


FIG. 8.—Gypsum from Middle West; 55 per cent Plaster, 45 per cent Water, Small Amount of Wood Chip Filler. Second Burn of Specimen.

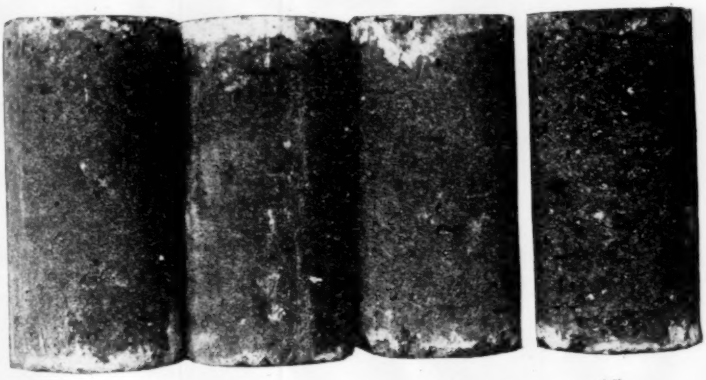
It is also to be considered that in all probability the thermal conductivity of the material next the surface was lowered by dissociation, calcium oxide being more porous than calcium carbonate. The higher loss in weight from the limestone-concrete specimens, and the higher strength in compression tests, are consistent with the temperatures shown. These findings differ somewhat in regard to the limestone concretes,



(*a*) (*b*) (*c*)

(*a*) Limestone Concrete.
(*b*) Gravel Concrete.
(*c*) Cinder Concrete.

FIG. 9.—Characteristic Appearance of Burned Specimens.



(*a*) (*b*) (*c*) (*d*)

(*a*) Trap-Rock Concrete.
(*b*) Slag Concrete.
(*c*) Cinder Concrete, 1 : 3 : 6.
(*d*) Cinder Concrete, 1 : 0 : 6.

FIG. 10.—Concrete Specimens after Burning.

from the conclusions of Professor Woolson.[1] Professor Woolson's work was done with comparatively small specimens, 4-in. cubes and 6 by 6 by 14-in. prisms. This would tend to result in a high proportion of the limestone being decarbonated in the tests at the higher temperatures. In his work in 1906,

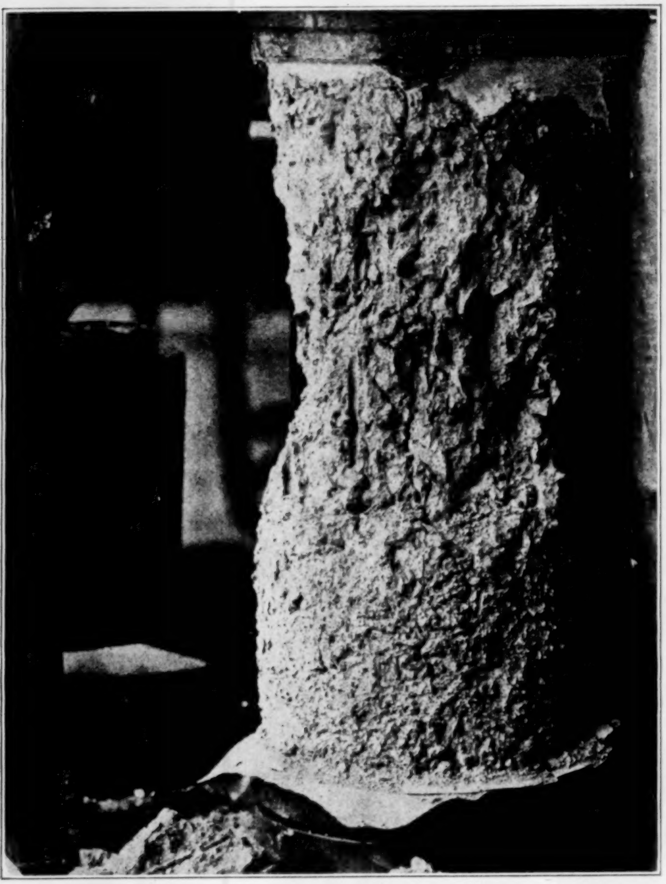


FIG. 11.—Limestone-Concrete Specimen after Compression Test.

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[1] "Investigation of the Effect of Heat upon the Crushing Strength and Elastic Properties of Concrete," *Proceedings*, Am. Soc. Test. Mats., Vol. V, p. 335 (1905); also "Investigation of the Thermal Conductivity of Concrete and the Effect of Heat upon Its Strength and Elastic Properties," *ibid.*, Vol. VI, p. 433 (1906).

the furnace temperatures did not exceed 815° C. (1500° F.), at which there would be practically no decarbonation of limestone and consequently no temperature check therefrom. It is also to be noted that part of the results in the 1906 investigation were favorable to limestone as against trap rock.

Among the gypsums, there are no wide differences in the temperatures attained by specimens from the different materials and mixtures represented, but there is a fairly consistent tendency for the heavier, denser specimens to show lower final temperatures than the more porous ones. This is presumably due to the fact that the heavier specimens were capable of absorbing more heat, both by dehydration and by the sensible heat taken up in the rise of temperature of the residual material by reason of its greater thermal capacity. None of the differences in the

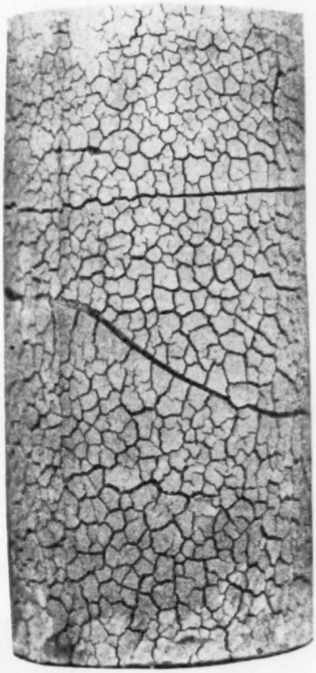
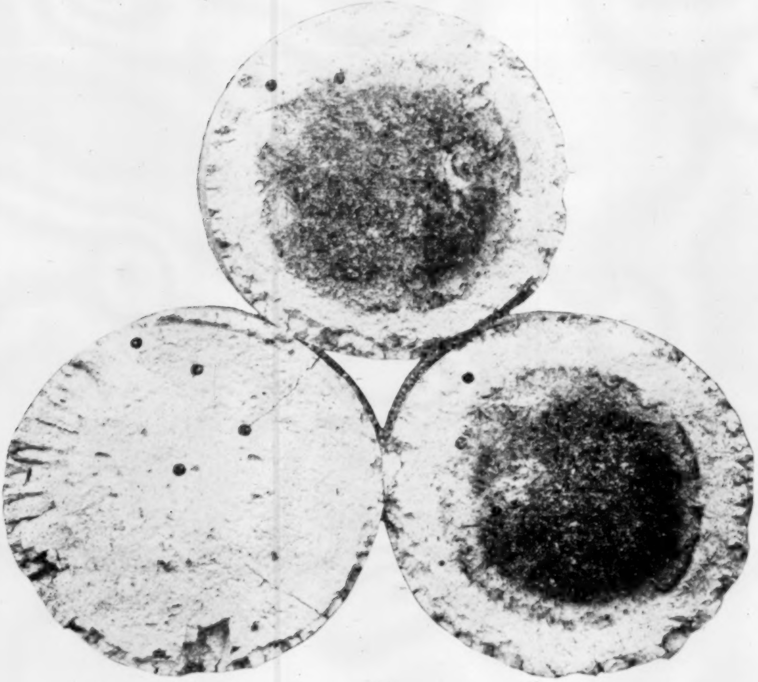


FIG. 12.—Gypsum Specimen after Burning.

temperature showings of the gypsums should be attributed to the different fillers used, because the quantities of material included as filler were apparently very small.

All specimens were examined after test for information on the extent of injury suffered by each. There was no satisfactory comparison possible among the clays, because all were cracked to some extent in kiln burning. There was no spalling in the tests.



(a) With Asbestos Filler. (b) With Cut Excelsior Filler.
(c) With Cut Excelsior Filler.

FIG. 13.—Gypsum Specimens after Burning.

Among the concretes, certain tendencies were apparent. All the gravels were very weak, the 1 : 3 : 6 mixtures somewhat more so than the 1 : 2 : 4, and the specimens in which 15 per cent of cement had been replaced by lime hydrate were still weaker then the others. Limestone concrete showed the highest crushing strength after test. The concrete specimens which stood handling best, after test, and even after repeated burns, were those made with an aggregate of selected, fused cinders containing very little loose ash or combustible material. The coarser masses had been crushed. This produced an aggregate which was of distinctly better grade than would be obtained in ordinary practice from bituminous coal refuse. Cinder concrete specimens made with an aggregate which had been prepared by passing ordinary, bituminous coal cinders over a ½-in. screen to remove the fines, crushing the large pieces, both coke and fused cinder, and including the resulting fines in the aggregate, were

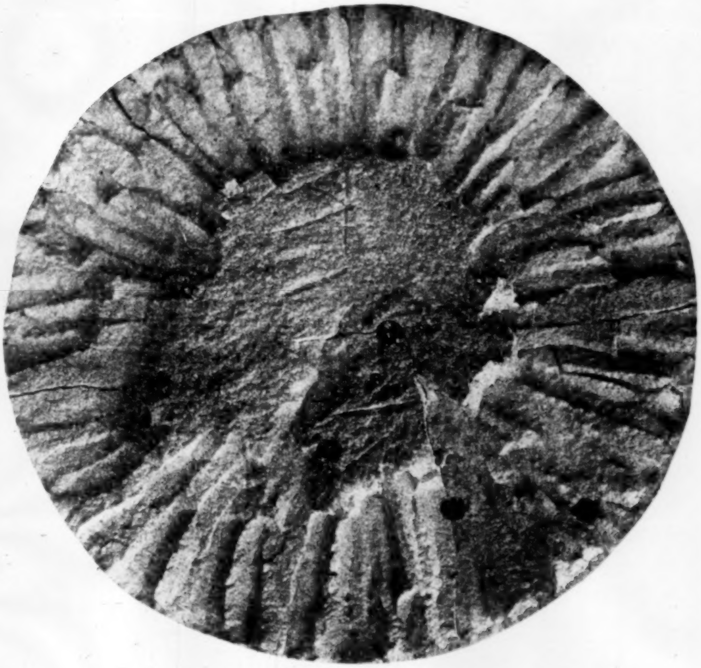


FIG. 14.—Gypsum Specimen after Burning; no Filler.

very weak and friable, apparently on account of too great a proportion of fine aggregate. Cinder concrete made from cement and unscreened, pile-run cinders with the large pieces crushed, the mix being 1 cement : 6 cinders, without sand, was fairly strong after test, although it made a particularly bad showing, thermally, on account of large voids and a large amount of combustible material. The slag and trap-rock concretes were tender and easily disintegrated by slight shocks or blows. Two of the limestone concretes that had been stored in the damp closet were cracked and weak, apparently due to being too green when tested.

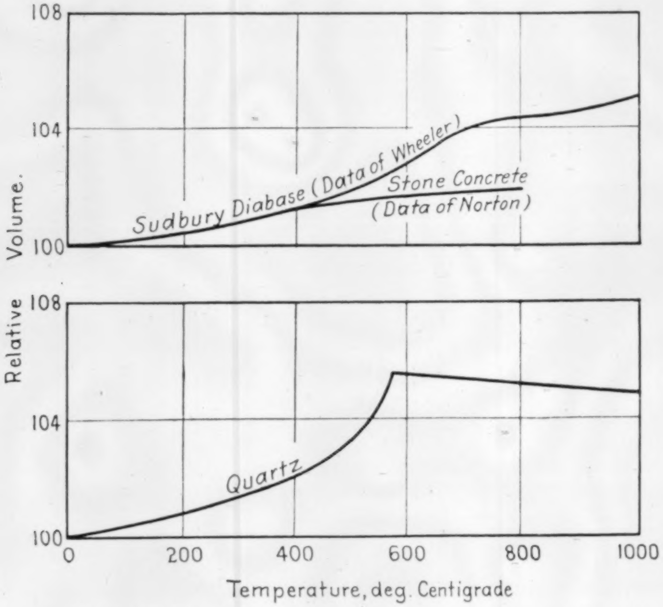


FIG. 15.—Volume Changes of Quartz, Diabase and Stone Concrete.

All gypsums were weak after test. No consistent tendencies could be discovered excepting that the series which had no filler was perhaps weaker than the others.

Characteristic appearances of burned specimens are shown in Figs. 9 to 14, inclusive. Fig. 11 shows a limestone concrete specimen after the compression test. The specimen shown in Fig. 9(*a*) was broken in handling.

The appearance of the gravel concrete specimens, considered in connection with observations recently made in tests of reinforced-concrete columns, indicates that the weakness of the gravel concrete specimens after test was due, at least in large part, to the shattering effect of the stresses caused by the sudden, high expansion of quartz which occurs at a temperature of 575° C., at which the inversion from Alpha to Beta quartz takes place. The magnitude and suddenness of this expansion is shown in Fig. 15, which shows, for comparison, the corresponding volume changes for diabase and for stone concrete. The diabase curve shows the expansion behavior of a material similar to our trap rock and the concrete curve is for 1 : 2 : 5 concrete, the stone probably being trap rock. The quartz and diabase curves are from the work of Day, Sosman and Hostetter.[1] The curve for concrete is from the expansion data of Prof. Chas. L. Norton.[2] The expansion behavior of quartz marks it as a material which would be likely to cause trouble in concretes in which it formed a large part of the aggregate, which is the case in most of our gravels.

CONCLUSIONS.

The tests as a whole indicate that there is not a very great difference among concretes from the aggregates commonly used, in respect to the protection that would be afforded to steel reinforcement embedded in concrete near the surface, assuming that the concrete protection did not come off and expose the reinforcement. It would not be wise to draw conclusions on that point from this work. As to protection of steel members, the insulating properties of the denser clays are indicated to be inferior to the more porous clays and to the concretes. It is to be noted in this connection, that this work deals exclusively with solid materials, and it is not attempted to draw comparisons between hollow blocks or tile of one material and a solid mass of another. The more porous clays compared fairly well with the concretes. It should be stated that the porosity of the more porous clay specimens was undoubtedly due in part to lighter burning in the kiln.

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[1] "The Determination of Mineral and Rock Densities at High School," *American Journal of Science*, Vol. 37, p. 1.

[2] "Some Thermal Properties of Concrete," *Journal*, Am. Soc. Mech. Engrs., June, 1913,

Gravel concretes gave particularly unfavorable indications. While the ground covered in this work alone is by far too narrow to permit of drawing hard and fast conclusions, the indications are consistent with the results of both of Professor Woolson's investigations previously referred to, and with information imparted to him by members of the British Fire Prevention Association, as well as with the report made by him on a concrete failure in a recent fire at Far Rockaway, N. Y. Hence, there is strong evidence that some gravels, at any rate, are distinctly inferior for concrete for fire-resistive construction.

Cinder concretes from bituminous coal cinders are apparently to be viewed with suspicion, especially if the cinders contain much combustible material, which is apt to be the case with cinders from bituminous coal. No anthracite cinders were included in this investigation.

Gypsums made good heat-retarding records but were very soft and weak after test. There was a favorable showing made by the denser, as compared with the more porous mixtures, which is consistent with theoretical considerations.

DISCUSSION.

Mr. Perrine. MR. HAROLD PERRINE.—In Mr. Hull's paper, mention is made of the use of bituminous cinder concrete in his investigations, and of the fact that no anthracite cinders were included. I should like to say a few words about the use of anthracite cinder concrete.

In our experience in New York City, which is based upon actual construction and also upon 50 to 100 full-size load and fire tests, we have found that anthracite cinder concrete is a dependable structural material. In tests which have come under the speaker's immediate observation, cinder concrete specimens, taken from slabs which had been subjected to a temperature of 1700° F. for 4 hours followed by a cold water application of 10 minutes duration, showed a strength equal to that of similar specimens cast under identical conditions but not subjected to a fire and water test. Both averaged about 1100 lb. per sq. in. in compression, a fair value for anthracite cinder concrete. The anthracite cinder used in New York City averages from 10 to 15 per cent of coal, determined by carbon loss on ignition. This percentage is sometimes as high as 25.

Anthracite cinder concrete is used in about 50 per cent of the floor areas being installed in New York City at the present time. It has been built on spans up to 8 ft. for many years. Approval was previously granted by the Building Bureaus for cinder concrete systems based upon their behavior in load tests. The new Building Code, however, departs from the former practice in that it permits of the installation of these slabs on the basis of engineering design as well as upon load tests. Recently, approval has been granted for a certain reinforced-concrete floor system using anthracite cinder concrete upon unlimited spans.

Mr. Slater. MR. W. A. SLATER.—Some tests have been made under my direction on gypsum blocks of a different size and shape from the specimens used by Mr. Hull. The blocks to which I refer were 4 in. thick and 12 in. square, and heat was applied to

Mr. Slater. only one side instead of both sides, allowing the side opposite to the fire to be exposed to the laboratory temperature and have the same effect in carrying away the heat that a partition might be expected to have. The result was that the temperatures were lower than the temperatures found in the tests reported here. I presume Mr. Hull will recognize that that difference might be found and that these tests should be used for comparison of one material with another rather than actual performance to be found in a partition.

There is a little difference in my findings as to the effect of the amount of water used in the specimen. The conclusion at which I arrived was that the amount of water in the specimen had practically no effect on the heat-transmitting properties. This may possibly be due to the fact that most of my specimens contained smaller amounts of water than those of Mr. Hull, the largest amounts used in my tests being about the same as the smallest amounts used in Mr. Hull's tests.

Mr. Hull. MR. W. A. HULL.—I should like to reply briefly to Mr. Perrine. Considerable information has been published on cinder concrete as a fire-resistive material, much of it being the results of tests and observations made in New York City and referring to cinders from anthracite coal. Inasmuch as anthracite cinders are not available in large quantities except in a limited section of the country, there may be some danger that conclusions based on the behavior of concrete from anthracite cinders may be applied to cinder concrete in general. As stated in the paper, the cinder concretes included in this investigation were all made with cinders from boilers using bituminous coal. These conclusions should not be applied to concretes from anthracite cinders.

It may be well to emphazise the point brought out by Mr. Slater that temperatures attained at given depths in these cylindrical specimens are higher than would have been the case at the same depths in blocks or slabs heated on one side only, and that the data given here should be regarded as purely comparative between different materials tested in the same way.

In regard to the effect, in gypsum, of the proportion of plaster to water, there appeared to be a belief, on the part of some of the gypsum manufacturers, that the insulating properties of

Mr. Hull. light, porous gypsum would be better than those of denser material, by reason of the lower thermal conductivity of the former. The results obtained in this investigation indicate that the advantage is slightly the other way within the range of mixtures included in this work. These results, taken in conjunction with those which Mr. Slater reports, indicate pretty conclusively that there is nothing to be gained, thermally, by sacrificing strength to obtain porosity.

Mr. Strehan. MR. G. E. STREHAN (*by letter*).—A study of the paper leads one to infer that the object in view was simply a comparative study of the thermal conductivity of structural materials under conditions that eliminated all variables other than the materials themselves.

The conditions of the tests are not comparable to actual building conditions; the specimens were protected on both ends with materials of low thermal conductivity and were entirely surrounded by heat waves and uniformly heated by convection and radiation. The results, if considered as absolute and definite values, would indicate that few and possibly only one of the commonly accepted fireproofing materials are satisfactory for the purpose.

The Joint Committee Report on Concrete and Reinforced Concrete accepted at this meeting by the Society recommends a 2-in. shell of concrete for the fire protection of columns. In view of this the data shown in Fig. 5 of the paper are extremely interesting. The author reports that limestone concrete gave the best results of the concretes. Yet, after two hours of fire, a $2\frac{1}{2}$-in. protection of limestone concrete transmitted sufficient heat to establish a temperature at this depth of approximately 350° C., and at the end of three hours of approximately 600° C., the latter temperature being considerably on the dangerous side of the critical temperature for steel.

The fire tests made on full-size fireproof building units for the New York building authorities in the years 1896 to 1915 outrank the tests made anywhere else both in number and importance. Of these tests some 50 to 100 were conducted on anthracite-cinder-concrete floor constructions and indicate that this material was superior to all others for the protection of structural members. It shows a higher conductivity, in common

Mr. Strehan. with all concretes, than some of the other materials, but is superior in lasting qualities, is more porous and possibly possesses a lower expansion rate than any other concrete.

It is regrettable that the author did not include concrete made of anthracite cinders in his investigation. The results of the New York tests can be found in the 1912 Proceedings of the International Association for Testing Materials.

The author finds that limestone gave better results than other concretes. In the tests above noted, a limestone concrete slab tested July 13, 1905, failed due to the deep pitting of the concrete and the exposure of the reinforcement. The value of such concrete as an insulator and protector is questionable. It is not merely a question of heating a mass of concrete, but of protecting embedded steel which lies within one or two inches of the surface in the usual concrete construction.

The whole matter of concrete construction is under fire in view of recent failures, and there is a crying necessity for accurate and scientific study of materials for this purpose. It may develop, as the author indicates, that expansion is the determining factor in fixing relative values of fire-resisting materials.

Mr. Boeck. MR. P. A. BOECK (*by letter*).—There are certain points in connection with the development of the insulation of high-temperature furnaces and other high-temperature equipment which might possibly throw some light on the subject under discussion.

The relation of thermal conductivity to fire-resistive construction is one which I believe is not sufficiently recognized generally by engineers and it is a matter which has been given comparatively little investigation. It is not sufficient to have a material refractory enough to withstand the temperatures encountered as a fireproofing material, on account of the fact that the heat which may be transmitted through the fireproofing may possibly do as much damage to the load-bearing member as if no refractory were present; in other words, fireproofing should carry with it the idea of a heat-resisting material and not simply incombustibility.

This point has been brought out strongly in many of the disastrous fires which have occurred in concrete and steel structures where the load-bearing members were not sufficiently protected from sudden flash or rapid momentary rise in temperature.

Mr. Boeck.

Let us consider for a moment the physical properties of a material which would have ideal physical and mechanical properties for use as a filler for binding materials, such as cement, plaster, lime, clay or similar materials. The primary idea in this class of material is to introduce voids or air cells by means of which insulation and light weight are obtained.

The size of these voids should of course be as small as possible to diminish losses by radiation across the voids and to prevent convection currents being set up by the circulation inside of the voids, both of which have a tendency to increase the thermal conductivity of the aggregate.

With these facts in mind the filler should have the following properties:

1. It should include the greatest practical number of the smallest possible voids.
2. The material itself should have the lowest possible thermal conductivity.
3. The filler should be sufficiently refractory so that high temperatures will not cause shrinkage or other physical change.
4. It should be sufficiently strong mechanically so that the filler when properly bonded or cemented together may be used for heavy construction work.
5. The coefficient of expansion should be as nearly as possible that of the binding material to prevent cracking and rupture with rapid temperature change.
6. It should be as light in weight as possible consistent with the strength required.
7. The aggregate should be sufficiently open and elastic to resist temperature changes effectively, that is, sudden or violent changes in temperature should not cause it to crack, check, spall or become weakened.

In work with high-temperature furnaces used in metallurgical lines a mixture has been developed which answers most of these requirements fully. This mixture has been in use for a considerable period in doors, bases and side walls of furnaces and has withstood continual use under hard working conditions without failure in any case, to my knowledge.

For door construction a mixture of coarsely ground calcined celite (a trade name for highly siliceous diatomaceous material)

Mr. Boeck. from which all the shrinkage has been eliminated, is mixed with a bonding material such as Portland cement or fireclay, depending upon the character of the equipment in which it is to be used, and the strength which is desired.

The material which has given the most satisfactory results for furnace-door construction consists of celite screened through a two-mesh screen and bonded by 10 to 12 per cent (by volume) of Portland cement. These are thoroughly mixed dry and sufficient water is added to give the necessary plasticity; the mixture is then rammed into the furnace doors or bases to give the required strength. This is allowed to remain moist for about 48 hours, then heated up slowly during the initial heat until the operating temperature is obtained.

This mixture gives a light-weight insulating lining which is not subject to spalling or cracking at temperatures of 2200 to 2500° F.

In one case, to the writer's knowledge, the interior temperature of a furnace to which a door insulated in this manner was attached, was maintained at about 2100° F. for a period of 48 hours, and the outside of the door, which was lined in this case with 4 in. of the celite mixture mentioned, was so cool that the hand could be borne on the surface with entire comfort.

It is possible that some mixture of this nature could be of service to Mr. Hull in the consideration of the heat-resisting properties of construction material. It would be of great interest to determine the actual thermal conductivity of these mixtures at various temperatures, in order that some quantitative results might be had.

Mr. Woolson. MR. IRA H. WOOLSON (*by letter*).—The conclusions deduced by Mr. Hull from the results of his experiments, merit thoughtful consideration by all who have to do with construction involving the use of heat-insulating materials for fire-resistive purposes. There are on record a large number of fire tests upon the common materials thus employed, but considering the fact that these investigations have been in progress for twenty years, there is a remarkable paucity of information regarding the relative heat-insulating properties of these materials when tested under identical conditions. The investigations covered by this paper furnish a valuable addition to that particular subject.

Mr. Woolson.

The author frankly states that it would be unwise to draw definite conclusions from the limited data presented. Nevertheless, the results are sufficiently significant to be indicative of what it might be reasonable to expect, in a general way, from more extended study.

The relative heat-insulating values of dense and porous terra cotta as derived from this investigation are in accord with recommended practice based upon previous experiments. The merits of other materials are generally confirmatory of other investigations, though these tests were undoubtedly made more accurately than former ones.

The temperature curves for the bituminous cinder concrete are not given, but from the description of the tests it would appear that the material did not give as satisfactory results as have previously been obtained from anthracite cinder concrete.

Assuming the "danger point for steel" to be at 600° C., as stated in the paper, and with the furnace temperatures employed, which were by no means excessive, the tests indicate that a protection of $1\frac{1}{2}$ in. of terra cotta or concrete would shield embedded steel from dangerous heat for two hours only. Such an interpretation may be misleading, however, for in practice the material would ordinarily be heated on one side only, and the heat would therefore be more or less diffused through the mass, thus lengthening the time necessary to raise the temperature of the steel to the "danger point."

The heat penetrations obtained are considerably more severe than the results of similar tests on concrete conducted by the writer in 1907, and reported in the Proceedings of this Society for that year[1]. However, the furnace temperature of those tests was an average of 1500° F., or 200° F. lower than that employed by Mr. Hull. Furthermore, the specimens were not heated equally on all sides as was the case in this investigation. These two variations in conditions might readily account for the difference in heat transmission.

My own investigations upon the loss of strength in steel due to heat would indicate that the "danger point" in tension (that is, loss of 50 per cent of strength) would be reached

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[1] "Investigation of the Thermal Conductivity of Different Concrete Mixtures and the Effect of Heat upon their Strength and Elastic Properties," *Proceedings*, Am. Soc. Test. Mats., Vol. VII, p. 404 (1907).

Mr. Woolson. at a temperature in the vicinity of 540° C., or 60° C. lower than assumed by the author. A lowering of this "danger point" would naturally call for a corresponding increase in thickness of protecting material to resist specified heat for a definite period of time.

I am much interested in the author's criticisms upon gravel concrete, particularly since he used a sandstone gravel. I have long been on record as opposing quartz gravel as a concrete aggregate for fire protection work, but am surprised to learn that sandstone gravel has also given unsatisfactory tests. The tests of quartz gravel concrete, upon which I based my criticisms, are described in the 1907 report previously mentioned. The report also contains a theory for the disintegration of the gravel concrete based upon the excessive relative expansion of quartz as compared with feldspar (the principle constituent of trap rock), also upon the unequal expansion of quartz along its different crystallin axes. The Sosman curve of expansion for quartz, as shown by the author (Fig. 15 of his paper), is new to me and extremely interesting. It may contain the full explanation of the phenomenon of disintegration of quartz gravel concrete when subjected to high temperatures. The behavior of quartz gravel concrete in the Far Rockaway fire seemed to justify my previous criticism of that material, and this feature was discussed at some length in my report upon that fire.

The heat-resisting properties of the gypsum specimens are shown to be very high, but are in accord with a similar elaborate investigation conducted by the Underwriters' Laboratories in Chicago in 1912.

It is exceedingly fortunate that the Bureau of Standards recognizes the need for such investigations, and that the work has been placed in such competent hands. It is earnestly hoped that facilities for future work may be provided in order that the investigations may be made so complete as to eliminate, as far as possible, all questions of uncertainty. A comprehensive investigation conducted under such auspices would be of inestimable value to the building public.

Mr. Hull. MR. HULL (*Author's closure by letter*).—In connection with Mr. Strehan's statements regarding the performance of anthracite cinder concrete in fire tests of full-size building units, record

Mr. Hull. should be made of the fact that the bituminous cinder concrete specimens included in this investigation of heat-insulating properties showed a toughness, after test, that was not found in specimens of any of the other concretes. Since it goes without saying that the effectiveness of any protective covering depends on its ability to stay in place when exposed to fire, this toughness may be an important indication. The fact should not be overlooked, however, that there is great variation in the character of bituminous cinders. It would not be safe to assume that a given concrete mixture, with bituminous cinders as the coarse aggregate, would be a satisfactory fire-protective concrete. If the specifications of the Joint Committee on Concrete and Reinforced Concrete were followed in the use of bituminous cinders, most of the variables would be eliminated, but the cost of cinders meeting these requirements would probably be excessive in most localities.

In connection with Mr. Boeck's discussion, it may be said that the ability of various protective coverings to protect load-bearing structural members from heat is being investigated to a considerable extent at the present time, but such investigations will probably be confined mainly to those forms of protection which are in actual use in building construction. The production of new building materials is perhaps not wholly without the proper scope of such investigations, but it is realized that among the other qualifications for a building material must be included that of moderate cost and it is perhaps for that reason that the use of kieselguhr in structural work has not been given more consideration.

The writer does not fully agree with Mr. Boeck in regard to his statement that many disastrous fires have resulted from insufficient protection of load-bearing members. There have been such fires, but some of them at least have been in buildings of obsolete types in which load-bearing members either were not protected at all or other conditions were such as to permit fires of great severity. It is perhaps not going too far to say that we have very few disastrous fires in buildings in which modern fire-resistive construction has been consistently carried out.

With reference to the fact that, as Mr. Woolson points out, the penetrations shown by the results in my paper are high as

Mr. Hull. compared with those obtained by him with furnace temperatures somewhat lower than those followed in this work, the size and shape factors undoubtedly tend to give rapid penetrations in cylindrical specimens, particularly those as small as 8 in. in diameter. This may compensate, approximately, for the lower danger point for steel which is indicated by his investigations. It should also be taken into consideration that the rate of temperature rise in these tests was slow as compared with the present tentative standard for fire tests and it seems quite likely that further investigation may show a covering of 1½ in. of concrete to be entirely inadequate for the protection of small steel reinforcement, especially column hooping, in fires of long duration.

FAILURE OF A 30-IN. TILE DRAIN AT ALBERT LEA, MINNESOTA.

By R. W. Crum.

SUMMARY.

A study of the adequacy of the Standard Specifications for Drain Tile[1] of the American Society for Testing Materials, and of the inadequacy of the types of specifications for drain tile in use prior to 1913, is presented by means of an analysis of the failure of tile in a large drain at Albert Lea, Minn.

The conclusions drawn are as follows:

1. Exact requirements for physical properties of large-size drain tile and sewer pipe, and provision on the job for testing the shipments, are always necessary.

2. The width of the trench in which a pipe is laid is an important factor in determining the necessary supporting strength of the pipe.

3. The present Standard Specifications for Drain Tile of this Society would have been adequate in this case to secure pipe of sufficient strength, provided Section 36 of the Specifications, requiring the use of tile having the standard ordinary supporting strength shown by Table II of the specifications, had been enforced.

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[1] 1916 Book of A.S.T.M. Standards, p. 452.

FAILURE OF A 30-IN. TILE DRAIN AT ALBERT LEA, MINNESOTA.

By R. W. Crum.

A line of 30-in. outlet drain tile laid in 1913 and 1914, partly in the city of Albert Lea, Minn., presents an interesting study of the inadequacy of the drain tile specifications which were in common use at that time and the adequacy of the specifications which were adopted later by the American Society for Testing Materials.[1] The original tile failed under the application of the back fill, and it was found, in December, 1916, when a lawsuit concerning this drain came to trial, that the heavier pipe which were put in later had also cracked into four segments longitudinally, in the typical manner.

A brief description of the drainage district is as follows: The system drains a marsh area of about 800 acres contiguous to Albert Lea, Minn. The outlet drain consists of 4400 ft. of 30-in. tile. This 30-in. drain crosses a number of railroad tracks, passes under a sanitary sewer, and a number of water and gas mains. The gradient is 0.03 per cent. The maximum cuts on the 30-in. line are 19 ft. at a railroad crossing, and 14 ft. on crossing a high point to avoid detour. Both of these cuts are less than 300 ft. of deep cutting. Iron pipes were used under railroad crossings. The normal cuttings were seldom less than 5 ft., except near the outlet of the 30-in. main.

The Standard Specifications for Drain Tile of this Society would have required the following physical properties of tile to be used in this location:

Minimum average ordinary supporting strength, lb. per lin. ft..	3000
Ordinary supporting strength for tile in cuts deeper than 6 ft., as shown in Table II of the Standard Specifications for Drain Tile: Minimum required for 12-ft. fill (assuming breadth of ditch = 41 in.), lb. per lin. ft..........	4850
Maximum absorption, per cent........................	7
Freezings and thawings to be endured..................	16

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[1] Standard Specifications for Drain Tile (Serial Designation: C 4-16), 1916 Book of A.S.T.M. Standards, p. 452.

In contrast to these definite requirements the specifications used read as follows:

> "Fully vitrified, salt-glazed drain tile, free from defects, material to be in all respects the same as compose standard, salt-glazed vitrified sewer pipe of first-class quality in every respect, except without bells or sockets."

A brief history of the drain is as follows: Contract was let and work started in July, 1913, using a clay drain tile concerning which the engineer in charge of the work testified in substance as follows:

Many of the tiles appeared, by reason of their muddy condition, to be culls and rejects from other work. They were crooked, blistered and ill-shaped. Many were broken, and others showed spider cracks and laminations. The tile were salt-glazed but not vitrified, since the material could be cut with a knife. When loaded with from 4 to 6 ft. on top of pipes with a muck and clay soil, the tiles split into four segments longitudinally.

It was stated by the engineer in charge that practically all of the tile in cuts deeper than 5 ft. failed as soon as the back fill was placed. The failure was typical of failure under bearing load, cracks occurring on the top, bottom and sides of the tile. The cracking of many of the tile was accompanied by reports about as loud as pistol shots.

The work was finally stopped by the resident engineer and the contractor ordered to remove the defective tile. Upon receiving this order the contractor ceased work and refused to go on with his contract, making the claim that he had furnished tile to comply with the specification. A new contract was let to another contractor, who proceeded to remove the defective tile, and to replace them and continue with 30-in. double-strength second-grade sewer pipe. The rest of the system was then completed.

After the completion of the system the county, acting in behalf of the property owners, brought suit against the first contractor for the difference in the contract price between the two contracts, and for the penalty provided in the contract for failure to complete the work within the specified time.

The counter claim of the contractor stated, first, that the tile furnished were in accordance with the engineer's specification; and, second, that the said specification would not provide a tile strong enough to withstand the load due to the back fill. This case came to trial in December, 1916. The defendant's case rested almost entirely upon the claim that the engineer's specification was not adequate

It is evident that the engineer had it in mind to secure the best drain tile to be had in the market of the size specified and of the thickness of wall commonly used in that district. At that time, owing to the lack of standard specifications for drain tile of accepted authority and lack of common knowledge concerning the necessary "ordinary supporting strength," there was no uniformity as to weight and shell thickness for drain tile among the different manufacturers in that district. Manufacturing practice was based upon experience with small-size tile. Some manufacturers did at that time list so-called "double-strength" drain tile. The defendant's attorneys claimed that the specification did not require the use of the so-called "double-strength" tile. The evidence does not show that the engineer actually contemplated the use of double-strength tile, but it was claimed for the prosecution that it seems reasonable to expect that it would be necessary to furnish the very best drain tile to be had, irrespective of shell thickness, for the drain tile to be "fully vitrified, salt-glazed drain tile, free from defects, material in all respects the same as compose standard salt-glazed vitrified sewer pipe of first-class quality in every respect," especially in view of the fact that the shell thickness of the ordinary drain tile was considerably less than that of "standard" sewer pipe.

Although it might readily be possible thus to convince an engineer that this specification would require the use of the best material on the market, it is not so easy to convince the ordinary juror, who properly is more apt to base his verdict upon the letter rather than upon the intent of the specification.

The point of the whole matter of interest to members of this Society is this: Under the engineer's specification, a costly lawsuit was possible, no matter what the intent of the writer. This was a common specification at that time. If the present specifications of this Society had been available and properly used at that time, there would have been absolutely no chance for argument concerning the adequacy of its provisions.

The maximum cut on this drain was 14 ft. or about 12 ft. above the upper one-fourth of the tile. The soil was a heavy wet clay weighing about 129 lb. per cu. ft., as obtained by determination at the site. The necessary width of trench was 41 in. "Ordinary" methods of pipe laying would be anticipated. To provide a reasonable factor of safety, tile should have been furnished for this location having an "ordinary supporting strength" for use in wet clay and under ordinary conditions of pipe laying, of 4850 lb. per lin. ft., as specified in Table II of the A. S. T. M. Standard Specifications for Drain Tile.

The probable actual load on the tile under the 12-ft. fill can be computed by the following formula:

$$L=CWB^2,$$

in which L = the load in pounds per linear foot, C = the coefficient of loads on pipe in ditches (C is a function of the height of fill, width of trench, friction of material against sides of ditch, and ratio of lateral to vertical earth pressure), W = the weight in pounds per cubic foot of the ditch-filling material, and B = the width of the ditch in feet just below the top of the pipe.[1]

Solving this formula, it is found that the probable maximum load on the 30-in. pipe under the maximum fill of 12 ft. would be 3650 lb. per lin. ft. It should be noted in this connection that the "minimum average ordinary supporting strength" of 3000 lb. per lin. ft. for extra-quality drain tile, as specified in Table I of the A. S. T. M. specifications would not provide a tile that would not crack under the conditions imposed by this ditch. It seems, therefore, that the use of Table II of the Standard Specifications, giving standard ordinary supporting strengths of drain tile for ordinary sand and for thoroughly wet clay ditch filling materials, is a vital necessity in the Standard Specifications for Drain Tile.

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[1] This formula is discussed in the "Report of the Investigations on Drain Tile of Committee C-6" of this Society, published as *Bulletin No. 36* of the Iowa Engineering Experiment Station, Ames, Iowa.

The quality of the tile furnished for use by the first contractor is shown in Table I, which gives the record of tests on samples submitted to the Iowa Engineering Experiment Station for test.

The average ordinary supporting strength of the tile tested (excluding B 4, which was found to be a defective specimen) was 1056 lb. per lin. ft. According to Table II of the A. S. T. M. specifications such tile could be safely used under a 2-ft. fill. Using the formula $L = CWB^2$ quoted above, and solving for the height of fill necessary to rupture the pipe, we find that the pipe tested would be expected to fail under a back fill of 3.5 ft., after sufficient time had elapsed to bring the maximum load on the pipe. The evidence showed that the tile actually failed promptly under back fills of from 4 to 6 ft.

TABLE I.—TESTS SHOWING QUALITY OF TILE FURNISHED BY FIRST CONTRACTOR.

Specimen No.	Diameter, in.		Thickness, in.				Length, in.	Weight of Pipe, lb.	Total Load, lb.	Load + $\frac{5}{8}$ Weight of Pipe, lb.	Load, lb. per lin. ft.	Absorption, per cent.
	Vertical.	Horizontal.	Top.	Bottom.	Left.	Right.						
A1[a]	31.20	30.9	1.70	1.80	1.80	1.75	29.9	427.5	2700	2967	1187	4.8
A2[b]	31.25	31.2	1.70	1.80	1.75	1.75	29.8	434.0	2370	2640	1065	4.9
A4[a]	30.80	31.2	1.70	1.80	1.75	1.75	29.7	432.0	2430	2700	1091	5.1
B[a]	30.60	31.3	1.70	1.80	1.75	1.75	29.3	425.5	2230	2496	1023	4.1
B2[a]	30.80	31.8	1.70	1.85	1.80	1.80	30.3	446.0	2020	2300	911	5.9
B4[b]	31.50	31.6	1.80	1.85	1.80	1.80	29.9	331.0	1320	1590	636	6.5

[a] Medium lamination.
[b] Slight lamination.

For purposes of comparison the results of tests from the records of the Iowa Engineering Experiment Station given in Table II will serve to show the quality of similar tile to be had on the market prior to the time in question.

The results of some tests of 30-in. sewer pipe made prior to the time in question are given in Table III.

It is evident that the tile used by the first contractor, as shown by the test quoted above, fell far short of what might have been expected from tile of that size on sale in that district at

Table II.—Tests of Vitrified Salt-Glazed Tile.

28-in. Tile.

Tile from	Test No.	Diameter, in.	Average Thickness.		Length, in.	Weight, lb.	Applied Load, lb.	Bearing Strength, lb. per lin. ft.	Modulus of Rupture, lb. per sq. in.	Absorption, per cent.
			Top, in.	Bottom, in.						
Lehigh, Ia.	317	28.1	1.98	2.00	29.5	520	9 970	4 050	1 260	5.6
	318	28.3	1.98	2.00	29.6	465	10 330	4 200	1 300	4.8
	319	28.0	1.95	1.90	29.6	445	8 110	3 280	1 060	5.1
	320	28.4	2.00	1.98	29.8	490	4 860	1 960	620	4.5
Average.....	......	....	....	....	....	....	....	3 370	1 060	5.0
30-in. Tile.										
Lehigh, Ia.	321	30.0	2.00	1.80	28.4	465	8 280	3 480	1 430	2.8
	322	30.5	1.92	2.00	29.5	452	6 850	2 780	1 000	4.6
	323	30.0	1.90	2.00	29.3	452	10 850	4 450	1 640	3.1
	324	30.1	2.05	1.86	29.0	490	6 600	2 740	1 050	4.3
	325	30.0	2.10	1.85	29.5	455	8 610	3 500	1 360	4.0
Average.....	......	....	....	....	....	....	....	3 390	1 300	3.8

Table III.—Tests of 30-in. Vitrified Salt-Glazed Sewer Pipe.

From	Test No.	Bell.			Average Diameter, in.	Thickness of Wall.		Wt., lb.	Lgth., in.	Breaking Load.		Modulus of Rupture, lb. per sq. in.	Absorption, per cent.
		Lgth., in.	Internal Diameter, in.	Thickness, in.		Top, in.	Bottom, in.			Total, lb.	Lb. per lin. ft.		
St. Louis, Mo. "Single Strength"	392	4.0	36.5	2.0	30.7	2.3	2.5	790	40.5	16 600	4 920	1 280	4.30
	393	4.0	36.5	2.0	30.4	2.4	2.2	790	40.4	17 550	5 220	1 470	4.15
	394	4.0	36.5	2.0	30.4	2.5	2.2	790	40.0	14 300	4 290	1 200	3.80
	395	4.0	36.5	2.0	30.2	2.4	2.2	790	41.0	12 050	3 530	990	4.45
	415	4.0	36.7	2.0	30.4	2.28	2.3	790	41.0	14 850	4 350	1 140	......
Average.....	....	......	......	......	......	......	......	....	......	......	4 460	1 220	4.18
St. Louis, Mo. "Double Strength"	396	4.5	37.5	2.0	30.3	2.7	2.7	910	40.7	17 130	5 060	960	4.52
	397	4.5	37.5	2.0	30.2	2.6	2.7	910	40.4	19 030	5 680	1 150	4.60
	398	4.5	37.5	2.0	30.3	2.6	2.7	910	40.4	23 290	6 930	1 400	4.53
	399	4.5	37.5	2.0	30.2	2.6	2.6	910	40.6	16 470	4 900	990	4.96
Average.....	....	......	......	......	......	......	......	....	......	......	5 640	1 120	4.65

that time. No engineer would claim that these tile passed the engineer's specification. But since there was no definite requirement as to ordinary supporting strength and since the tile were salt-glazed and the percentage of absorption not excessively high, attorneys could very easily make this a debatable point.

When the broken tile were removed, the whole line of 30-in. pipe was relaid with No. 2 double-strength sewer pipe. It developed at the trial of the case that many of these sewer pipe had cracked under fills of from 10 to 12 ft. The claim was therefore made by the defendant that since double-strength sewer pipe were not strong enough to carry the load, the engineer's specification (which did not definitely prescribe the use of such pipe) was not adequate. In considering this point, it should be remembered that on account of the bells of the sewer pipe, it was necessary to lay this pipe in a wider trench than that for drain tile, and that the pipe would therefore have to support a heavier load than the tile.

Using, again, the formula

$$L=CWB^2,$$

in which $C=1.96$, $W=129$ lb., $B=3.86$ ft., height of fill at the location of the failure = 10 ft., we find that the load on the sewer pipe was probably 3770 lb. per lin. ft., while the load on the drain tile in the same location would have been 3230 lb. per lin. ft. The advantage of using double-strength sewer pipe was, therefore, lost in a large measure, due to the fact that the sewer pipe must carry more load.

The writer has taken five sewer pipe typical of the so-called No. 2 double-strength sewer pipe from the ditch and tested them for supporting strength and percentage of absorption in accordance with the standard A.S.T.M. methods for testing drain tile, and in a similar manner to the tests reported in this paper. The results are shown in Table IV.

The estimated load from the ditch filling material in this case is 3770 lb. per lin. ft. The average strength of three typical pipe taken from the ditch was 3597 lb. per lin. ft. The factor of safety is therefore 0.95. From these figures we would expect many tile to be broken under the 10-ft. fill and some to be intact. Twenty-seven of the sewer pipe out of 41 inspected were cracked.

The results of these tests confirm the writer in his conclusion that it is possible to estimate the ditch loads on tile and pipe with accuracy, and that therefore specifications should be used requiring pipe which will sustain a load equal to the estimated load times a reasonable factor of safety.

Two supplementary reasons for the cracking of these tile have been advanced:

1. The center line of the tile line in question is only about 15 ft. from the center line of the Chicago, Milwaukee & St. Paul Railway. Engineers for the defendant advanced the theory that the vibration in the soil set up by the moving trains while the back fill was still fresh was sufficient to overcome the friction of the ditch-filling material against the sides of the trench and thus cause the tile to support the whole weight of that material. This would amount to a load of 5420 lb. per lin. ft.

TABLE IV.—TESTS OF 30-IN. CLAY SEWER PIPE.

Tile No.	Bell.			Average Diameter, in.	Thickness of Wall.		Wt., lb.	Length, in.	Breaking Load.		Modulus of Rupture, lb. per sq. in.	Absorption, per cent.
	Lgth., in.	Internal Diameter, in.	Thickness, in.		Top, in.	Bottom, in.			Total, lb.	Lb. per lin. ft.		
1	5.00	36.37	1.60	30.69	2.45	2.45	670	29.75	6069	2450	794	6.7
2	4.50	.....	1.60	30.87	2.50	2.45	734	30.62	7863	3080	981	6.7
3	4.50	36.75	1.60	30.56	2.55	2.50	738	30.25	8863	3520	1047	5.7
4	4.50	36.75	1.60	31.00	2.55	2.50	753	30.25	8863	3520	1068	7.3
5	4.50	37.25	1.60	30.81	2.50	2.50	750	30.00	9363	3750	1123	7.00
Ave.	....	.....	....	.....	....	....	...	.....	....	3597	1079	6.66

NOTE.—Only Nos. 3, 4 and 5 enter into the averages, Nos. 1 and 2 being excluded for the following reasons:
No. 1.—After breaking, it was found that fracture occurred at an old crack about 9 in. long at spigot end. Also about 2 ft. of the edge of the bell had been broken off at some previous time.
No. 2.—About 2 ft. of the entire bell was broken off in handling.

2. It seems more reasonable to the writer to take into account the fact that, when the original tile were replaced with the No. 2 double-strength sewer pipe, there were numerous cave-ins of the trench walls, which undoubtedly resulted in increasing materially the width of the ditch at the levels of the

top of the pipe at the points where the cave-ins occurred, and which hence undoubtedly greatly increased the load on the tile at the places where they occurred, since the loads increase with the square of the increase in width of trench. This explanation is supported by the fact that the failure occurred in connected series of tiles, and not in single specimens. Starting at the manhole from which the inspection was made, pipe Nos. 1 to 5 were not cracked, 6 to 9 were cracked, 10 to 16 were not cracked, 17 to 28 were cracked, 29 and 30 were not cracked and 31 to 41 were cracked. The report of the engineer who made this inspection states that: The tile were cracked on top, and some side cracks were noted. The cracks in these tile were very irregular, while the cracks in the tile first used were nearly straight. The cracks were reasonably near the center line, the maximum distance from the center line being about 7 in.

It is hoped that we will be able to make a further investigation of the exact effect of the vibration due to trains and of the actual load on the tile.

The general conclusions the writer has drawn from the study of this failure are as follows:

1. That exact requirements for physical properties of large drain tile and sewer pipe, with provision on the job for testing the shipments, are always necessary.

2. That we have here a striking illustration of the fact, already demonstrated theoretically and experimentally, that the width of the ditch in which a pipe is laid is a very important factor in determining the necessary supporting strength of the pipe.

3. That the present Standard Specifications for Drain Tile of this Society would have proved adequate in this case, if the manufacturer or contractor had been held to deliver pipe having the standard ordinary supporting strength specified in Table II of the specifications, as required in Section 36 of the specifications, as follows:

> "The manufacturer or other seller shall not be held responsible for cracking of drain tile in ditches unless by special agreement in advance, and in any event his obligation shall be held to be discharged by the delivery of drain tile having the minimum ordinary supporting strengths specified

in Table II; and, if it is not otherwise specified in advance by the purchaser, tile shall be supplied of the strengths specified for clay ditch filling, for "ordinary" pipe laying and for widths of ditch at the level of the top of the tile to 0.5 ft. greater than the outside diameter of the tile. The purchaser shall furnish to the manufacturer or other seller complete information, in advance of receiving bids, as to the number of linear feet of drain tile of each diameter required for each different depth of ditch, measured to the nearest foot from the surface of the ground to the top of the tile."

I might add that the jury disagreed in the trial of the case, and no verdict was rendered.

DISCUSSION.

The Chairman. THE CHAIRMAN (PAST-PRESIDENT A. N. TALBOT).—It is not often that the sufficiency of a specification receives such prompt confirmation as in this case. I believe it is of value to have experiences of this kind put on record so that we can check up on the work which is being done by the Society. The paper is open for discussion.

Mr. Brooks. MR. BENJAMIN BROOKS.—The term "defective pipe" is used in this paper. In one place the author states that a defective pipe was removed from the ditch and other pipe was put in.

If I read the paper correctly, the purport is that the pipe itself was not defective but that the specifications were at fault, and that a pipe that is admirably fitted for one location may not be serviceable in another. I am particular about this point because the specifications of the Society for Drain Tile are new and I should like the manufacturers to feel that the specifications are not intended simply to rule out some of their pipe, but that they may with equal likelihood operate to their advantage.

I have in mind a case where a contractor in Texas used some 12-in. pipe in a ditch about 25 ft. deep, in quicksand, and after a number of cavings and widenings of the ditch, the pipe broke. The people who supplied the pipe and the contractor who placed it sent me the data to see if I could help them. By applying the formula which Mr. Crum has used to the widened and caved-in ditch, it was easy to show that the pressure on the pipe was about twice what it would have been if the job had been conducted correctly. In addition to that, on account of the quicksand foundation under the pipe, the contractor supported the pipe at one place underneath by planks instead of on a suitable bed including at least a quarter of the pipe. Accordingly, by applying the factor for a knife-edge load instead of a sand-bearing load on the pipe, the actual pressure on the pipe was found to be considerably over 200 per cent of what it would have been had the specifications of this Society for that kind of work been followed. To make the matter conclusive,

Mr. Brooks.

I then obtained a dozen representative pipes from the factory and tested them according to the A.S.T.M. specifications, and found that they were well over the prescribed breaking load. The presentation of these facts to the pipe manufacturer and contractor was sufficient to place the responsibility where it belonged,—on the contractor who attempted to use an unsupported 12-in. pipe in a ditch excessively widened by cave-ins, and 23 ft. deep. That was a case where the specifications worked to the advantage of the material men.

Mr. Crum.

MR. R. W. CRUM.—Perhaps the tile first used in this ditch described in the paper were of first-class quality, but I am inclined to think that they were defective for the reason that the tests show these tile to be much inferior in strength to other tile of the same size and class on sale in this district at that time. However, the data upon which this opinion is based are not very extensive. The comparative tests are shown in the paper.

Mr. Marston.

MR. A. MARSTON (*presented in written form and read by the author*).—The failure described by Mr. Crum is typical of numerous cases which come to the attention of the staff of the Iowa Engineering Experiment Station, at Ames, Iowa. In two other recent instances especially complete data were secured, and in each case they further confirmed the reliability of the formula $L=CWB^2$ for computing maximum loads on pipes in ditches due to filling materials, as well as the adequacy and reasonableness of Table II of the Standard Specifications for Drain Tile (Serial Designation: C 4–16).

FAILURE BY CRACKING OF 32-IN. CLAY DRAIN TILE IN JOINT DRAINAGE DISTRICT, NO. 33 BOONE CO. AND NO. 10 GREENE CO., IOWA, 1916.

Diameter of Tile, in.	32
"Ordinary Supporting Strength" of Tile, by Test, lb. per lin. ft.	1455
Supporting Strength of Tile as Laid, lb. per lin. ft.	about 1200
Width of Ditch at top of Tile, in.	46
Height of Fresh, Uncompacted Filling Over Pipe found to cause Cracking, ft.	6
Unit Weight of Fresh, Uncompacted Filling, lb. per cu. ft.	about 80
Calculated Weight on Tile, by Formula $L=CWB^2$, lb. per lin. ft.	1325
Safe Strength for Pipe Required by Table II of Standard Specifications for Drain Tile to carry 6 ft. of Compacted Clay Filling, lb. per lin. ft.	3400

Mr. Marston. FAILURE BY CRACKING AND COLLAPSE OF 24-IN. CLAY SEWER PIPE ON GRAND AVENUE, DES MOINES, IOWA, 1915.

Diameter of Sewer Pipe, in	24
Class of Sewer Pipe	Single Strength
"Ordinary Supporting Strength" of Pipe, by Test, lb. per lin. ft	2430
Supporting Strength of Pipe under the Actual Bedding Conditions, by Special Test, lb. per lin. ft	3440
Width of Ditch at top of Pipe, in	46
Weight of Compacted Sand Filling, by Test, lb. per cu. ft	123
Height of Fill above Top of Pipe, ft	12
Calculated Weight on Pipe, by Formula $L = CWB^2$, lb. per lin. ft	3700
Safe Strength for Pipe Required by Table II of Standard Specifications for Drain Tile to carry 12 ft. of Compacted Sand Filling, lb. per lin. ft	5670

SUGGESTED IMPROVEMENTS IN THE MANUFACTURE OF SILICA BRICK.

By C. E. Nesbitt and M. L. Bell.

SUMMARY.

In this paper the authors first point out the necessity of the cooperation of the investigator, the maker, and the consumer before much can be accomplished in the way of improvement of the product.

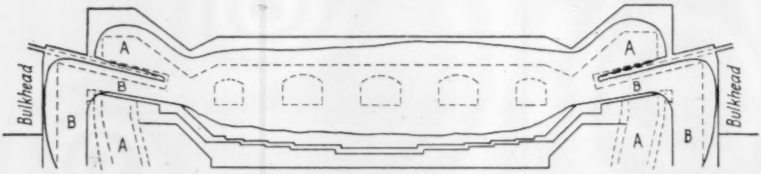
The present method of making brick is next outlined in a general way, followed by some experimental work which illustrates the importance of the degree of fineness to which the raw material is ground and the benefits which may be derived from power pressing.

The paper is then brought to a conclusion by pointing out the defects that increase the failures in actual service and how to keep them reduced to a minimum.

SUGGESTED IMPROVEMENTS IN THE MANUFACTURE OF SILICA BRICK.

By C. E. Nesbitt and M. L. Bell.

The study of silica refractories is receiving more and more attention as shown by the increased number of articles appearing in the technical magazines. The published literature so far has dealt mainly with the theoretical study of the raw materials, while the practical side has received very little attention. Theoretical information is of extreme importance, but its importance is negative until put to practical use. The brick manufacturer is not as a rule a man of theoretical training, hence theoretical investigations to be of use to him must be expressed in simple and explicit terms, easily understood. This does not however excuse the maker, who in most cases has been negligent in regard to investigations of the properties and improvement of his own product. The consumer has also been at fault in that he has not freely cooperated with the maker and informed him wherein the bricks failed. To obtain the best result it is necessary that these three—the investigator, the maker, and the consumer—should work together with heartiest cooperation.



A = Air Ports. B = Gas Ports.

Fig. 1.—Vertical Lines of Erosion on an Open-hearth Furnace.

In the manufacture of iron and steel, silica brick find their greatest use in the open-hearth furnace. The demand here is for a brick of high refractoriness which does not become soft or plastic at working temperatures. The silica brick fulfills these requirements in that it is refractory and does not yield to compression unless the stress is sufficient to crush it. The ability of silica brick to stand when only a small portion of the original wall is left is well illustrated in the bulkheads. (Fig. 1.) This wall when built is 18 in. thick, but wears away in spots to less than 1 in. in thickness and still the brick retains its place.

In the by-product coke industry the development of the use of silica brick is interesting. Formerly a quartzite brick was used which was made of a mixture of clay and ganister. The object of the quartzite brick was to overcome the shrinkage and settling common to fire brick. Some twelve years ago silica brick were used experimentally in both bee-hive and by-product ovens. The experiment proved so successful that their use increased rapidly; not only did their expansion on heating produce close, well-fitting joints and overcome settling difficulties, but their better conductivity greatly reduced the coking time. (Fig. 2.)

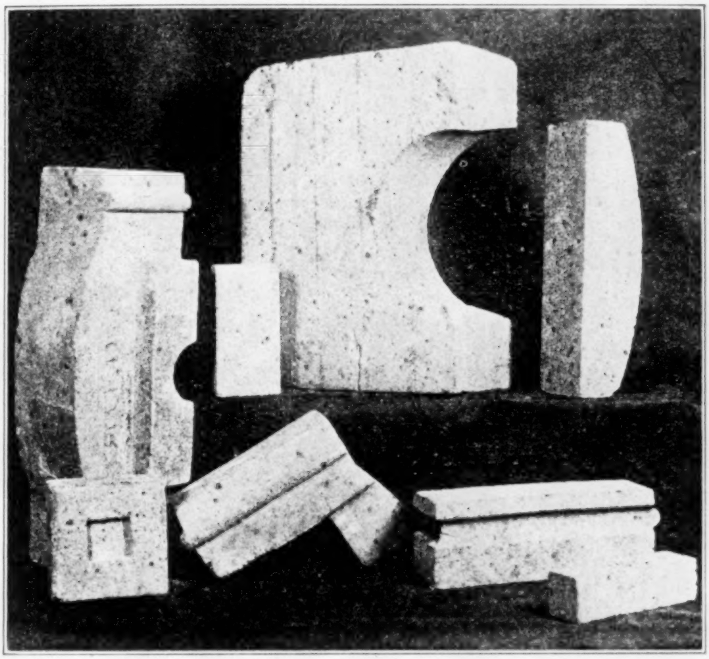


FIG. 2.—A Few Difficult By-product Coke-Oven Shapes, illustrating Shrinkage Cracks and Cracks due to Re-entrant Angles. Note 9-in. Brick in Center for Relative Size.

THE MANUFACTURE OF SILICA BRICK.

In the manufacture of silica brick in this country the raw material used is quartzite rock commonly known as ganister. The important deposits are found in Huntingdon and Blair Counties of Pennsylvania, Devil's Lake region of Wisconsin, and in Alabama and Colorado. It is essential for high-grade silica brick that the ganister be hard and dense. The rock should analyze about 98 per cent silica, with 1 per cent each of iron and alumina. The raw material is broken to convenient size, dumped into a wet pan, and ground with water and lime. The degree of fineness and the amount of water is left to the judgment of the operator. This is a source of considerable variation, as it will be shown later that the degree of fineness has considerable influence on the strength of the finished brick.

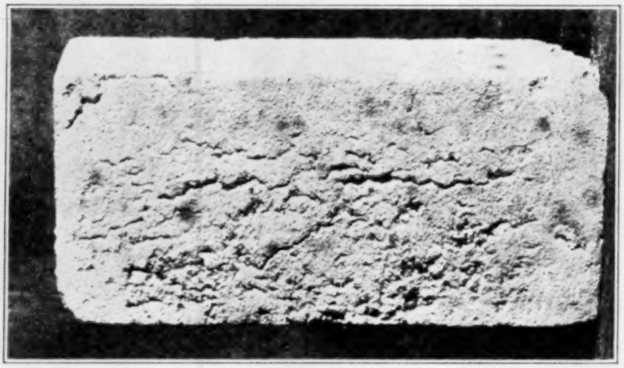


FIG. 3.—An Example of Bad Molding, Poor Slicking, and Sponginess.

The binder commonly used is lime. Two per cent has been found to give the most satisfactory results. Percentages greater than this lower the refractoriness and decrease the strength of the brick, while percentages below $1\frac{1}{2}$ per cent do not give a satisfactory bond. Binders such as iron, alumina, talc, water glass, and other substances have been tried with more or less success.

The amount of water necessary to produce a satisfactory working mud varies somewhat with the kind of rock used, amount of calcined material present and the method of manufacture. The percentage of moisture present is important. About 10 per cent of available moisture is the proper amount for hand-made brick. Too wet a mud will produce bricks and shapes which distort after being taken from the mold, while too dry a mud is responsible for a number of very serious defects, such as unfilled corners, sponginess, improper slicking, and lack of cohesion, all of which tend to give a finished product of low mechanical strength, very susceptible to spall, and slag penetration. (Figs. 3 and 4.)

The molding is a very critical point in the manufacture of silica brick. Improper filling of the molds, too wet or too dry a mud, improper slicking, dirty palette boards, or careless handling, all contribute their share to the making of a defective product. The defects can largely be eliminated by close inspection and rejection of all but good bricks. As an illustration, bricks were received from a certain company which were poor in quality. The spalling loss was very high, running from 50 to 60 per cent. An investigation was made of the methods at the plant and irregularities of mixing, molding, and burning were found. Careful inspection was applied at certain points in the brick manufacture and to-day this same plant, with no radical change in its general method, is producing bricks much more uniform in shape, strength, and appearance, and with a spalling loss of only 25 per cent.

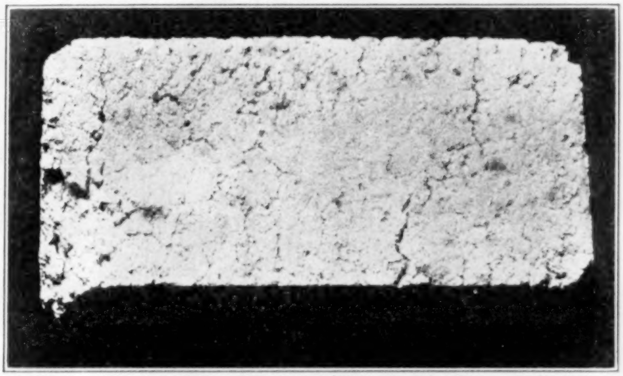


FIG. 4.—An Example of Bad Molding and Fire Cracking.

Drying of the brick after molding is accomplished either on a hot floor, heated by steam, or in tunnels heated by hot air from the kilns. Drying on the floor is naturally the slower of the two methods, 4 to 6 days being required for 9-in. brick and proportionally longer for larger shapes, while drying in tunnels may be accomplished in 18 to 24 hours. It is important that drying shall be done carefully and thoroughly, for unless the material is bone dry when placed in the kilns, fire cracks are apt to result. (Fig. 5.)

The dried brick are set in kilns, which are usually of the down-draft type ranging in capacity from 50,000 to 150,000 bricks. It is important in setting that the bricks shall be true to shape in order that they have a proper bearing and stack well. Green bricks will not sustain a load of much more than 100 lb. per sq. in., hence care should be taken not to overload them. Shapes should not be placed near fire boxes nor bear much weight. They are frequently boxed in.

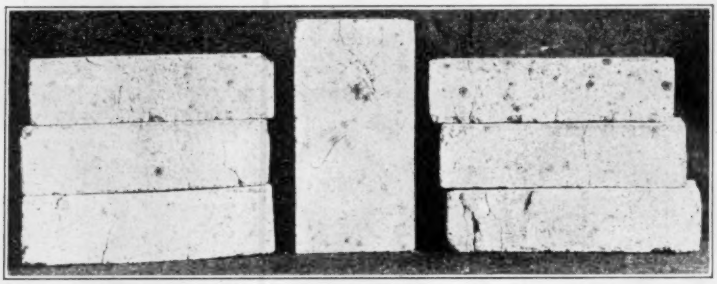


FIG. 5.—Characteristic Fire Cracks.

Complete burning requires from 10 to 15 days, with a gradual increase in temperature until at least cone 16 is attained (1450° C.). The final temperature must be held a sufficient length of time to thoroughly and uniformly heat the entire kiln, which may require anywhere from 1 to 3 days, depending on the size of the kiln.

The cooling usually requires about 5 days and is accomplished by gradually opening the fire holes and doors until the bricks are cool, when they are removed and placed directly on cars or in the stock house.

TESTS OF SILICA BRICK.

With a view to studying the effect of degree of fineness of material and pressure, an experimental series of silica brick was made with these two factors as the only variables. A good quality of Pennsylvania ganister was selected and ground dry in three lots to pass a 12, 8, and 4-mesh screen respectively. To the ground material was then added the water and lime so as to give 9 per cent of moisture and 2 per cent of lime. From each of the above meshes standard 9-in. bricks were made on a small hydraulic press at eight different pressures varying from 187 to 2500 lb. per sq. in. The bricks were dried, burned, and cooled, following regular silica-brick practice, and were then subjected to certain physical tests to determine density, resistance to impact, resistance to spall, resistance to corrosive action of slag, and expansion due to heating. The methods used are described in detail in the Appendix. A summary of the results is given in Table I, in which the results of the three meshes are averaged according to pressure.

TABLE I.—TESTS OF SILICA BRICK, ARRANGED ACCORDING TO PRESSURES.

Pressure under which Bricks were made, lb. per sq. in.	Apparent Specific Gravity.	Impact in.	Percentage Spalled.	Slag Penetration, sq. in.		Expansion, lin. in. per ft.
				Open-hearth Slag.	Heating-Furnace Slag.	
187	1.50	47	45.5	3.48	2.28	0.155
500	1.58	46	48.3	3.68	2.42	0.155
750	1.59	60	53.2	3.63	2.41	0.158
1000	1.63	65	51.4	3.79	2.42	0.160
1250	1.65	68	47.9	3.65	2.50	0.155
1500	1.66	65	43.6	3.82	2.50	0.160
2000	1.67	67	44.8	3.64	2.45	0.159
2500	1.68	71	42.0	3.72	2.50	0.160

Table II shows the results of the various pressures averaged according to mesh.

Table I indicates that little is gained by increasing the pressure. Table II, however, shows the importance of the selection of mesh. It will be observed that the strength as indicated by the impact test is greatest with the fine-ground material, while the percentage loss by spalling increases with fineness of mesh. Two very important properties in first-class silica brick are the mechanical strength and resistance to spalling; hence from the data we must select a mesh at the expense of one of these properties. More is gained by the coarse mesh, hence we advocate for silica brick a coarse-ground material. (Fig. 6.)

TABLE II.—TESTS OF SILICA BRICK, ARRANGED ACCORDING TO MESH.

Mesh.	Apparent Specific Gravity.	Impact, in.	Percentage Spalled.	Slag Penetration, sq. in.		Expansion, lin. in. per ft.
				Open-hearth Slag.	Heating-Furnace Slag.	
4	1.66	57	30.4	3.92	2.63	0.156
8	1.62	63	47.6	3.67	2.39	0.159
12	1.58	64	63.2	3.44	2.29	0.159

Table III gives the individual losses by spalling for each pressure and mesh.

The average spalling loss for hand-made silica brick is about 30 per cent. From Table III it will be noted that power-pressed silica brick were made with the same spalling loss from 4-mesh material pressed at 1500 lb. per sq. in. These bricks were true to shape, sharp-cornered, dense and had smooth, marble-like surfaces. Molding defects such as soft corners, sponginess, improper slicking, and lack of cohesion were all eliminated. The prospect of the adoption of power pressing as a step towards better and more uniform silica brick was clearly brought out

TABLE III.—LOSSES BY SPALLING, EXPRESSED AS PERCENTAGE.

Mesh.	Pressure under which Bricks were made, lb. per sq. in.							
	187	500	750	1000	1250	1500	2000	2500
4	21.9	34.9	37.9	38.7	31.4	30.4	19.1	29.1
8	51.6	45.9	62.6	51.0	43.3	34.8	38.3	43.3
12	62.9	64.0	59.2	64.4	69.1	65.8	67.0	53.6

by this series of tests. The idea of power pressing is not entirely new, yet little has been done in a commercial way. One of the large concerns is now manufacturing brick this way on a commercial basis. The average spalling loss of these bricks is 25 per cent. (Fig. 7.)

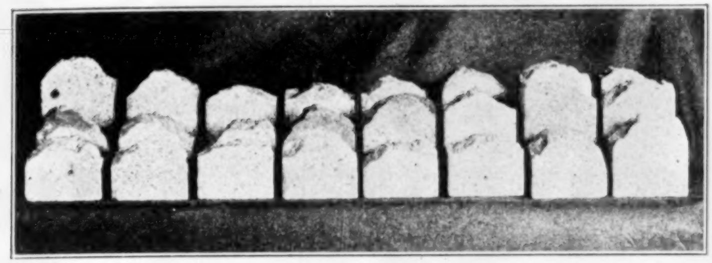


FIG. 6.—Spalling Test on Experimental Series. Note Bricks in Illustration are Averaged with Pressure Increasing from Left to Right and Mesh Increasing from Front to Back. (Tables I and II are arranged in corresponding order.)

In actual service, although well guarded as far as practice and construction will permit, silica brick fail from three primary causes: spalling, crushing, and slagging. These cannot be entirely eliminated, but by a deeper study of the manufacturing of brick they can be reduced to a minimum. An ideal brick

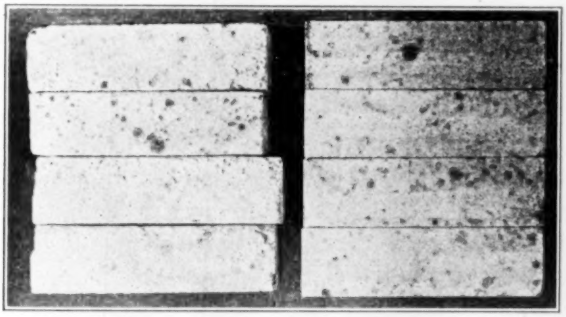


FIG. 7.—Comparison of Hand-Made and Commercial Power-Made Bricks. Note Joints.

must be well molded, true to shape, thoroughly bonded, and of good mechanical strength. (Fig. 8.)

A visual inspection of almost any shipment will show at least 20 per cent of defective brick. Six shipments of brick of different brands brought out the fact that 60 per cent of the defects were due to molding, 30 per cent to fire cracks, and 10 per cent to improper setting and irregular shapes. These defects can be largely overcome in the present method of manufacture by careful inspection and rejection of the improperly made bricks at certain points in the manufacture.

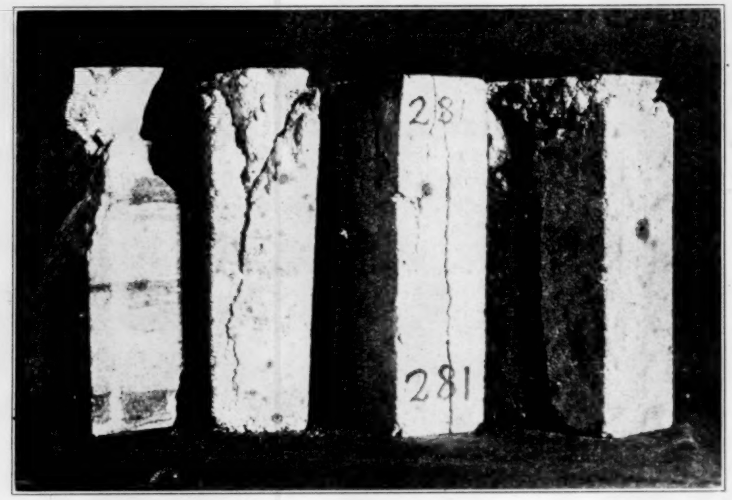


FIG. 8.—Characteristic Crushing Tests.

The ganister must be carefully selected, and sorted; all rock that is soft or which carries considerable iron, clay, sandstone, or other foreign matter should be rejected. (Fig. 9.) The ganister selected should be ground to just pass a 4-mesh screen, care being taken to avoid an excessive amount of finely ground material. To insure uniformity, frequent sieve tests of the mud should be made. To secure a sufficient bond, 1.75 to 2 per cent of lime should be used. In order to avoid irregular shapes and molding defects, 9 to 11 per cent of water should be used for hand-made bricks and the material should be pounded into the mold. The bricks should be thoroughly dried so as to avoid fire cracks when placed in the kiln. Great care should be used when heating, especially during the first part of the burn, to prevent fire cracking; this applies also to the cooling. The bricks as they are removed from the kiln should be carefully inspected and all bricks showing molding defects, fire cracks, and irregular shapes should be rejected.

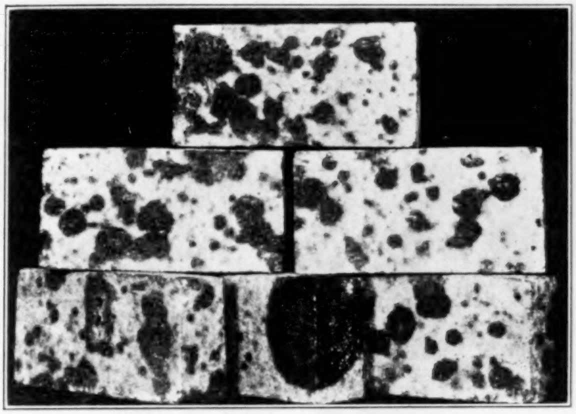


FIG. 9.—Defects Due to Iron in Raw Materials.

The consumer should be allowed to inspect the bricks before they are loaded on the car and a place provided for storage of such brick as he may desire to test.

APPENDIX.

IMPACT TEST.

Construction of Apparatus.—The machine for determining the resistance to impact consists of two vertical uprights 15 ft. high, supporting two vertical and parallel $1\frac{1}{2}$-in. angle irons. The angle irons are placed so that their enclosed angles face each other and are braced $3\frac{3}{8}$ in. apart measured from the inside of the angles, so as to allow a ball $2\frac{1}{2}$ in. in diameter to fall freely between them, the sidewise movement of the ball not to exceed $\frac{1}{4}$ in. These angle irons are adjusted so that bricks of different heights may be placed under them and are marked, beginning at the lower ends, so as to clearly designate successive heights of 2 in. The test specimen is placed directly below the angle irons on a steel block of sufficient size to give a solid foundation. The upper face of the block is perfectly smooth and at right angles to the angle irons. The block is approximately $6\frac{1}{2}$ by 15 by 12 in. and weighs 332 lb. The falling weight is a steel ball $2\frac{1}{2}$ in. in diameter, weighing 2.34 lb.

Operation of Test.—The brick to be tested is first carefully inspected and, if necessary, ground on a carborundum wheel so that it sets firmly on one of its ends. On the other end of the brick the diagonals are then drawn to locate the center. The brick is placed in a cold furnace and heated at a uniform rate from atmospheric temperature to 1350° C. (The heating as a convenience is done over night, at a uniform rate not to exceed 260° C. per hour. The total time of heating should in no case be less than 6 hours.) The final temperature of 1350° C. is maintained for not less than 2 hours.

The brick is then ready for the test and is placed on the steel block, so that the ball between the angle irons touches the brick at its marked center. Iron blocks are placed around the brick until they are nearly level with the top of it, to prevent sidewise movement, but the brick is not wedged. The ball is then dropped from a pair of tongs on the end of the brick at successively increasing heights of 2 in. until the brick fractures. Any fracture radiating 1 in. or more from the point of contact of the ball is considered a fracture, and the distance in inches of the last drop is the result of the test.

CRUSHING TEST.

The apparatus may be any of the standard testing machines such as the Olsen, Reihlé, or others.

Operation of Test.—The brick to be tested is first carefully inspected and, if necessary, ground on a carborundum wheel so that both ends are parallel. The brick is then measured, placed in a cold furnace and heated at a uniform rate from atmospheric temperature to 1350° C. (The heating as a convenience is done over night at a uniform rate, not exceeding 260° C. per hour. The total time of heating should in no case be less than 6 hours.) The final temperature of 1350° C. is maintained for not less than 2 hours. The brick is then quickly removed from the furnace and placed on end in position on the testing machine. To insure good bearing a thin piece of asbestos board is placed above and below the brick. The machine is started and operated until the brick is crushed. The crushing strength is recorded in pounds per square inch.

SPALLING TEST.

Operation of Test.—Preparatory to the test the furnace is heated to 540° C. and held at this temperature for about one hour. The bricks to be tested are dried at 100° C., weighed, and then placed in the doorway of the furnace, their ends flush with the inside of the wall. In case a sufficient number of bricks are not being tested to entirely fill the doorway, the remainder of the space is filled in with other bricks to insure heating the ends only. The bricks are then heated for one hour and plunged separately into 2 gallons of water at 20° C. to a depth of 4 in. and held there for 10 seconds. They are then removed and allowed to dry for 3 minutes in air. The bricks are then placed in the doorway of the furnace as before and the operation repeated for a total number of ten times. They are then dried at 100° C. for at least 5 hours and all particles which can be easily broken off with the fingers are removed. The bricks are then weighed and the percentage of loss calculated to the original weight.

SLAGGING TEST.

Operation of Test.—In preparing the sample for the slagging test, the 9 by $4\frac{1}{2}$-in. unbranded side of the brick is bisected by a line across the narrow dimension and diagonals drawn on each half. At each intersection of the diagonals a circular cavity is drilled $2\frac{1}{2}$ in. in diameter and $\frac{1}{2}$ in. deep at the sides. The drills are $\frac{5}{16}$ in. thick and $2\frac{1}{2}$ in. wide, measured diagonally across opposite corners, while the point of the drill includes an angle of 150 deg. A template is made of $\frac{3}{16}$-in. sheet steel to insure having all drills properly ground. To insure a standard depth of cavity, two inverted U-shaped pieces of metal are clamped to each drill when in use and adjusted so that the drill cannot enter the hole more than $\frac{1}{2}$ in., measured on the side of the drill. The cross-section of the cavity passing through the deepest point has an area of 1.7 sq. in., and is checked up for size and depth by a template. The pressure used in drilling must not be so severe as to partially crack the brick.

After the cavities have been drilled and cleaned of loose material the bricks are placed level in a cold furnace and heated as in the impact test. When 1350° C. is reached 35 g. of standard open-hearth slag are placed, by means of a long rod with a spoon-shaped end, in one cavity, and 35 g. of standard heating-furnace slag in the other cavity. The slags used shall have approximately the following analysis:

	OPEN-HEARTH SLAG.	HEATING-FURNACE SLAG.
Silica (SiO_2), per cent	18.4	35.0
Iron (Fe), per cent	14.5	44.0
Manganese (Mn), per cent	5.1	0.5
Alumina (Al_2O_3), per cent	3.8	6.0
Lime (CaO), per cent	44.1	1.5
Magnesia (MgO), per cent	6.3	0.5
Sulfur (S), per cent	0.4	....

The fineness of the slag is such as to pass a 40-mesh sieve. The bricks are kept at a temperature of 1350° C. for 2 hours after the slag is added, at the end of which time the furnace is allowed to cool, the bricks being left in the furnace. The bricks when cold are sawed lengthwise at a right angle to the unbranded side, so that one side of the cutting wheel passes through the center of the drilled cavity, and this area of slag penetration, together with the cavity is measured with a planimeter. The area of the original cavity is subtracted, giving the result in square inches of slag penetration. The wheel used for sawing the brick is a carborundum wheel 12 in. in diameter, $\frac{1}{8}$ in. thick, $1\frac{1}{2}$-in. center hole, grit 16, bond NCH.

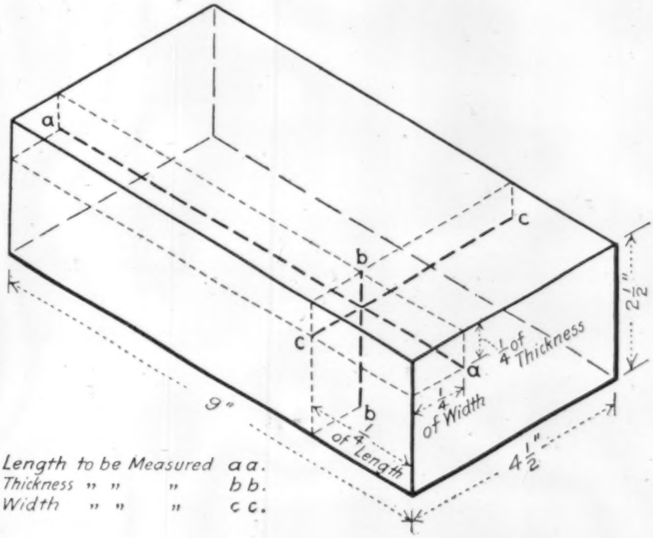

a
c
b
c
b
a
$\frac{1}{4}$ of Thickness
$2\frac{1}{2}''$
$\frac{1}{4}$ of Width
$\frac{1}{4}$ of Length
9"
$4\frac{1}{2}''$
Length to be Measured aa.
Thickness " " " bb.
Width " " " cc.

FIG. 10.—Axes along which Measurements are Made in Determining the Size of a Brick.

EXPANSION.

Construction of Apparatus.—The machine used for the measurement of expansion consists of a micrometer mounted on a steel post, which in turn is mounted on a steel plate 24 by 6 by $\frac{1}{2}$ in. To the stem of the micrometer is fastened a steel disk 1 in. in diameter and $\frac{1}{8}$ in. thick. In line with the micrometer is a second steel post on which is mounted a $\frac{1}{4}$-in. sliding steel rod with a similar circular disk, as with the micrometer. The rod can be adjusted so as to determine measurements up to 14 in. To insure the proper setting of this rod, standard bars 3, 5, 9, 11, and 13 in. long are used.

Operation of Test.—In preparing the brick for the test its ends are ground so as to be parallel to each other at right angles to the sides. The length is then measured cold. The brick is then placed as in the impact test, removed from the furnace and measured hot. Removing from the furnace and measuring usually requires about 30 seconds. The results are calculated to inches per linear foot.

SIZE.

Operation of Test.—The size is determined by the use of the micrometer described under expansion. All measurements are made while the brick is at atmospheric temperature. Small particles of sand, or other foreign matter adhering to the brick are removed before measuring. The locations for taking the measurements are shown in Fig. 10.

DISCUSSION.

Mr. Unger.

MR. J. S. UNGER (*presented in written form and read by the author*).—Any information regarding bricks is of interest to both the producer and consumer in these days of high-price bricks and a brick shortage. As the paper just read deals with suggested improvements in silica bricks, I have some suggestions to make for the manufacturer and user of bricks. Silica bricks have replaced fire-bricks in many industries, and their field is constantly growing. Where it was formerly believed to be impossible to use any other than fire-bricks, as in a lime kiln, or coke oven, silica bricks have partially or wholly supplanted them with a pronounced improvement in service. The brick maker has made very little study of the use and effect of raw and calcined ganister. In most cases, he does not know how much water is added to the pug. It is rarely determined and still less is known of its effect. The fineness of grind is determined approximately only. Regular sieve tests are not made. Proper adjustment of the coarse and fine particles is not studied. Comparative tests of dry-pressed and hand-made bricks are rarely made nor have the several methods been carefully compared to show their influence in producing bricks true to shape, size and freer from warping, bad molding, slicking or other defects.

I do not wish to do any one an injustice, but there is a tendency to let the other fellow do the experimenting. The consumer does not make comparative tests by trying out a new brand of bricks with one with which he is familiar, in the same furnace and at the same time. He tries out different bricks in different furnaces, at different locations and times, but does not obtain information of value. He has his ideas of what he thinks constitutes a good brick, but he rarely proves them. One man wants a soft brick, another for the same purpose a medium burn, and a third a hard burn. A user may build a furnace roof of large bricks arched in one direction, at another time a dome-shaped roof of smaller bricks for the same furnace, and judge the brick quality from the difference in the life obtained.

Mr. Unger. Silica bricks, in fact any brick, may be injured by heating up a new furnace too quickly. Mortars and cements of inferior quality may ruin good bricks. Some believe a mortar should vitrify or partially fuse to make a good joint, forgetting that its bonding power after becoming cold is not a criterion of what it may be when hot or in actual service. If it softens or fuses in service, the weight of the superimposed brick work will squeeze it out of the joint and throw strains in the brick work which may ruin it. I have seen good bricks ruined in this way. The ideal mortar should have the same qualities as the bricks it binds together.

A marked improvement in brick manufacture can be made by the adoption of standard shaped brick. The Refractories Manufacturers' Association began this work four years ago and their efforts to date have been very encouraging. An engineer may design a brick of intricate shape without consulting the maker whether it can be successfully made. Different engineers will design different shapes for one and the same purpose. The common feather edge, side and end skews, neck brick and the split are old ceramic crimes. Intricate shapes for fireboxes, by-product coke ovens, boiler settings, etc., with knife edges, tongues and grooves, pockets, holes and of small cross-section at one point and large cross-section at another, are examples of bad design. The intricate expensive mold, the retarded production, the excessive breakage in manufacture and the slow delivery mean a high extra cost which is borne by the consumer. In many cases such shapes could be made of better quality and lower cost if made in two or three pieces. There is a belief that it is cheaper to use a shape than cut a brick. This is not always true, especially if but few bricks are needed. This is a problem for the chief brickmason and the maker to decide. Simplicity in design, quality of production and cost should decide the question of shape brick.

The laboratory testing of bricks must be studied from different angles to be of value. Any test proposed must have a practical value. A test should imitate service conditions or show a relation between the laboratory and the practical results. A test which requires considerable time to complete is of little value, as the bricks during a period of brick shortage will be in actual service before the test is completed. A brick must possess other than heat-resisting qualities. No general tests can be prescribed, as the tests should vary as the requirements of service change. Chemical composition is not as important as some believe. Two bricks may be alike in composition but quite different in physical properties. Some tests are of greater importance than others. In a silica brick, for instance, the spalling loss and crushing strength are probably of the greatest value to determine the quality. These two tests will show up the bricks with defective interiors caused by internal cracks, fissures or laminations. Careful visual inspection will largely eliminate defective bricks. A good cook will taste the food during its preparation. Can the brick maker not adopt this old rule by removing a brick from the kiln, from time to time during the burning, and from its examination determine whether the temperature should be raised or lowered or the time lengthened or shortened to produce the best brick? The maker can do a great deal without special equipment by making tests in his kilns imitating the service conditions of the bricks to determine what is the principal cause of their failure. The consumer can aid by good workmanship, good design, better operating conditions, careful comparative tests and a study of the causes for brick failures with applied remedies for their prevention. **Mr. Unger.**

Much has been done in the last ten years, but it is not enough. Bricks must follow the same general law of other manufactured products. They must be of better quality than formerly; they must be used, not abused; and the cost must come down.

MR. THOMAS NOLAN.—I should like to ask if these bricks have ever been used for outside work in building operations. The objections to them are the difficulties encountered in obtaining various surface colors and textures, and their lack of durability when exposed to the weather; but they certainly must be quite free from those elements which are liable to cause what is called "efflorescence" on brick surfaces. My understanding is that most if not all silica bricks are entirely free from magnesia. Am I right in this? **Mr. Nolan.**

MR. UNGER.—Silica brick could not be used for building purposes, owing to the fact that they do not weather very well. **Mr. Unger.**

Mr. Unger. They are very porous, as porous almost as a piece of lump sugar; they weather quickly and shatter. As far as efflorescence is concerned, I should say that a portion of the efflorescence might be dissolved by the rain and removed. As far as the magnesia content is concerned, there is practically none in the silica brick.

Mr. Stone. MR. G. C. STONE.—I have listened with much interest to Messrs. Nesbitt and Bell's paper and Mr. Unger's discussion of it. The paper describes some very interesting experiments and is a long step in the direction of the production of better refractories.

I was particularly interested in the experiment with different sized materials, the results of which are given in Tables II and III. I do not think that in this regard the test went as far as is desirable. Our own experience is that it is not simply the maximum size of the material but the relative amounts of the different sizes that count. We have found that the denser brick is the better for our purpose; and that the tensile strength is a fair measure of the density. In testing clays and grog, we make briquettes and determine their strength after drying and burning under definite conditions. The grog rarely contains the best proportions of the different sizes to give the maximum density, and our experiments show that by regrading the material the tensile strength can be greatly improved. In one case the screen analyses was:

SIZE MESH.	AS RECEIVED, PER CENT.	RECTIFIED, PER CENT.
10–16	33.5	24.2
16–25	18.1	14.0
25–40	17.9	16.5
40–60	10.8	10.2
60–100	8.4	10.3
100	11.3	24.8

The column "As received" gives the screen analysis of the material as produced by crushing. The column "Rectified" gives the proportion of each size that should be present to give the minimum of voids.

TENSILE STRENGTHS, LB. PER SQ. IN.

	AS RECEIVED.		RECTIFIED.	
Burnt 24 hours at 1000° C	270	233	341	301
Burnt 24 hours at 1200° C	284	...	376	...
Clay: Grog	7 : 9	6 : 9	7 : 9	6 : 9

Mr. Stone. The increase in strength varies from 26 to 32 per cent.

Such close sizing is not practicable or necessary, as the following experiment indicates:

SIZE MESH.	AS RECEIVED.	RECTIFIED.
10–16	42	24
16–25	22	26
25–30	9	
30–50	12	50
50	15	

Nine parts of these grogs mixed with 7 of clay and burnt at 1100° C.:

AS RECEIVED.		RECTIFIED.	
Tens. Str., lb. per sq. in.	Shrinkage, per cent.	Tens. Str., lb. per sq. in.	Shrinkage, per cent.
243	4.84	371	5.44

This corresponds to 52 per cent increase in tensile strength. The increase in shrinkage is probably due to less internal fissures.

In Messrs. Nesbitt and Bell's experiments, no screen analyses of the material are given, but we know that the coarser the material is crushed the smaller the proportion of very fine particles there are in the resultant product. We also know that there is usually a less proportion of fines than is requisite for maximum density. When the maximum size is smaller, the ratios of the different sizes would therefore probably be better. This is confirmed by the results of their impact and slag penetration tests, in both of which respects, the finer material is better. It is also noticeable that the coarser material shows considerably less spalling when made under higher pressure, while the fine shows practically no difference.

PERCENTAGE OF SPALLING.

	PRESSURE UNDER 1000 LB.	PRESSURE OVER 1000 LB.
4 Mesh	33.4	27.5
8 Mesh	52.8	39.9
12 Mesh	62.6	63.9

Higher pressure would naturally produce a greater increase in apparent density in material containing a maximum of voids

Mr. Stone. than in material containing a minimum. Spalling is not important in the application of refractories with which I am familiar and I have had no occasion to study its causes and prevention. I should, however, expect that the denser and stronger the material, the less the spalling would be, and I am therefore at a loss to account for the poor results shown by the finer material, which might be expected to be denser and stronger.

The whole subject of refractories is in a very unsatisfactory condition, and this I believe is as much the fault of the consumer as of the producer. Few users know what qualities are requisite and still fewer make regular tests. The main criterion is price. As long as this is so, and as long as refractories are bought without specifications or tests, the producer will make what he can make most cheaply. More than this—if a manufacturer ships a bad lot of brick that is rejected, he can almost always get it accepted by a near neighbor of the customer and loses nothing but a trifling amount of freight while that particular customer is subjected to delay in deliveries that may be very serious. To avoid this, he often accepts material that he knows is unsuitable. If the consumers will agree to test all brick and accept or reject it on quality, so that bad brick causes a loss to the manufacturer, we will soon see an improvement.

Our experience is unique, as we make a considerable amount of our own refractories and buy much more. Like all zinc smelters, we make our own retorts and condensers; but, unlike most, we also make our own furnace linings and have succeeded in increasing their life 200 to 400 per cent. Our experience is that the principal errors in making refractories are:

Improper selection of raw material;

Improper preparation of material;

Carelessness or ignorance in the preparation of the mix, particularly in regard to the amount of water used;

Insufficient mixing and pugging;

Poor or careless molding;

Too rapid or incomplete drying;

Irregular, often too low, temperature in burning.

I believe that no one will question that pressed brick, especially re-pressed brick, are, in the long run, superior to hand-made. Nevertheless, excellent hand-made brick are made

Mr. Stone.

and a large proportion of the special shapes and unusually large blocks are, and will be, hand-made, as they are not required in sufficient quantity to justify the expense of presses.

I must disagree with Mr. Unger in regard to the use of shapes. We find that shapes give us longer lived furnaces and can be laid more cheaply than standard brick. He mentioned many shapes that he called "ceramic crimes." The shapes are not the crimes, but rather the way in which they are often made. To make them properly costs more, but the saving in mason's labor and longer life of the furnace much more than compensates for it.

The question of the relative economy of using shapes and cutting depends on the conditions the brick has to stand in service and on the possibility of getting first-class masons. If good masons are available and the furnace is not liable to be damaged by corrosive slags or gases, cutting is probably cheaper; in all other cases I have found shapes best. If the life of the furnace can be materially prolonged by better brick-work, it is worth considerable extra first cost.

I believe the accompanying illustrations will show that many of the shapes we regularly make are difficult. Several come under Mr. Unger's class of ceramic crimes, but we have made all in considerable quantities for many years and find their use very advantageous and are able to build structures with them that could not be built with standard brick. All are hand-made brick, and an examination of our stocks will show that they compare well in accuracy of shape, uniformity, and soundness with any brick made. I agree with Mr. Unger that many shapes are designed that are objectionable. The necessary changes are not always in the direction of making more and simpler pieces. For instance, the large ring brick (No. 3) shown in the sketch is an example. As now made it is not especially difficult. If made in several pieces, it is quite as troublesome and much less satisfactory. If made in halves, it is impossible.

The brick with four holes (No. 1) is probably the most troublesome to mold. It is easily dried and burnt.

The long baker's tile (No. 10), on the contrary, is easy to mold but troublesome to dry and burn.

The heavy block (No. 5) is one of the worst of the lot, as

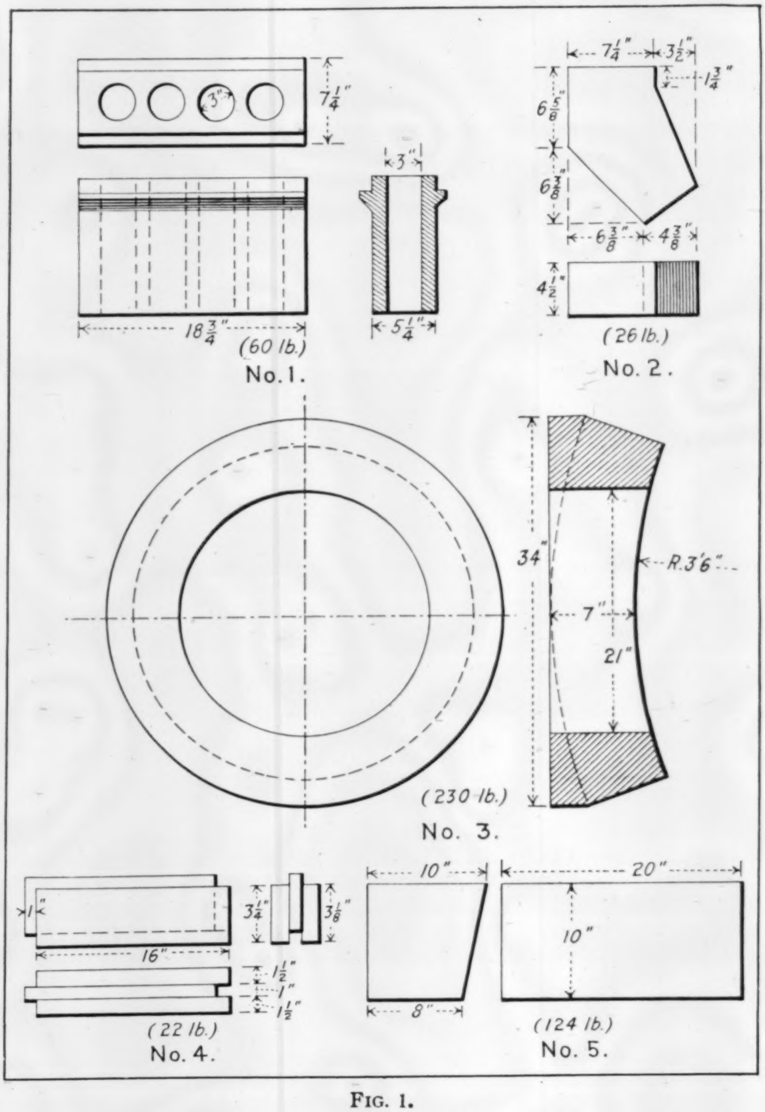

7 1/4"
3"
3"
18 3/4"
5 1/4"
(60 lb.)
No. 1.
7 1/4"
3 1/2"
1 3/4"
6 5/8"
6 3/8"
6 3/8"
4 3/8"
4 1/2"
(26 lb.)
No. 2.
34"
R. 3'6"
7"
21"
(230 lb.)
No. 3.
1"
16"
3 1/4"
3 1/8"
1 1/2"
1"
1 1/2"
(22 lb.)
No. 4.
10"
8"
20"
10"
(124 lb.)
No. 5.

FIG. 1.

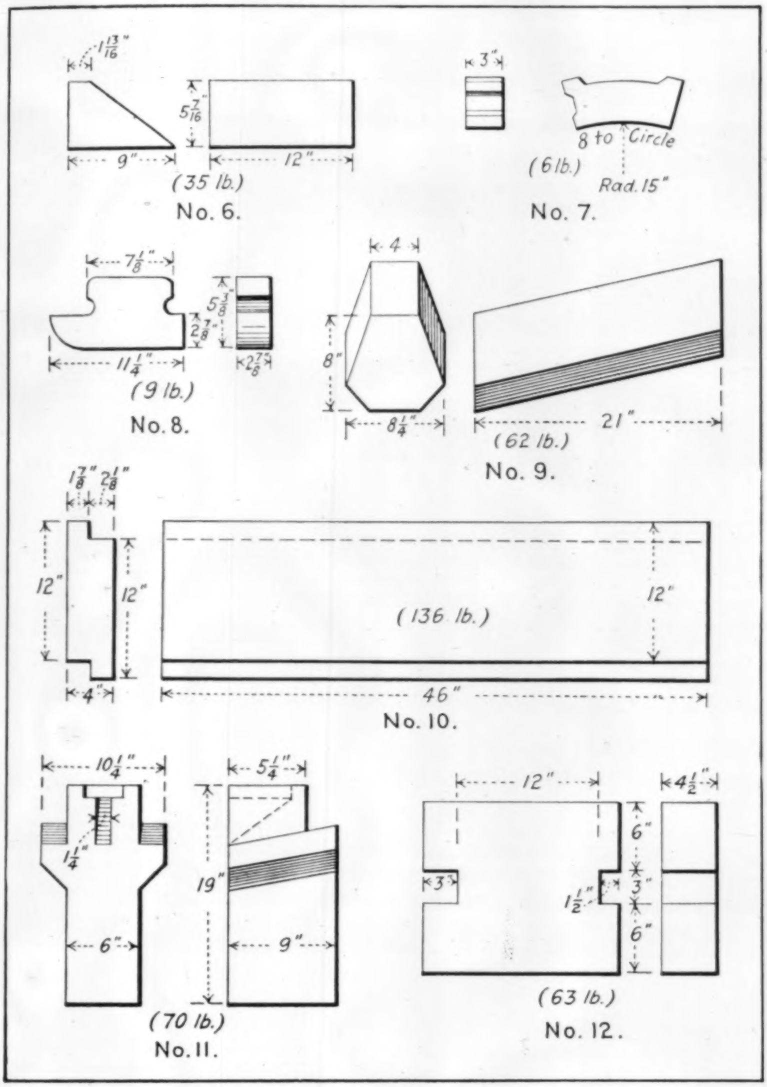

1 13/16"
5 7/16"
9"
12"
(35 lb.)
No. 6.
3"
8 to Circle
(6 lb.)
Rad. 15"
No. 7.
7 1/8"
5 3/8"
2 7/8"
11 1/4"
2 7/8"
(9 lb.)
No. 8.
4
8"
8 1/4"
21"
(62 lb.)
No. 9.
1 7/8" 2 1/8"
12"
12"
4"
(136 lb.)
12"
46"
No. 10.
10 1/4"
1 1/4"
6"
5 1/4"
19"
9"
(70 lb.)
No. 11.
12"
4 1/2"
6"
3"
3"
1 1/2"
6"
(63 lb.)
No. 12.

FIG. 1 (*continued*).

Mr. Stone. several months are required to dry it properly so that the sides will be straight and there will not be an excessive loss in burning.

The flue lining (No. 7) is not difficult to make and its use reduces the cost of lining flues and steel pipes very materially.

The arch block (No. 4) is troublesome to make and we could not find a brickmaker who would undertake it, so made it ourselves. With it we built an arch of 4 ft. 6 in. span and 3$\frac{7}{8}$ in. rise which stood well in a furnace for some years until the furnace itself became obsolete. When it was torn down the arch was in better condition than the other parts of the furnace. It would have been impossible to build this arch of the ordinary shapes.

A very common error is to suppose that the higher the temperature a brick will stand, the better it is for all purposes. In every case with which I have come in contact, a brick that will stand a couple of hundred degrees higher temperature than the maximum it will be subjected to in service has proved better for the purpose than a more refractory one.

I agree with Mr. Unger that there is a strong tendency to let the other fellow do the experimenting and that much of the experimenting is done in a way that does not give conclusive results. Until we all experiment and do it intelligently, the situation is not likely to improve. The great variations in the conditions under which brick are used make it difficult to write general specifications. Each user should develop his own tests to suit his own requirements. Tests should follow furnace conditions as closely as possible and should not require too much time.

I have already spoken of the faults of the brickmaker and it is only fair to mention those of the user, which are often as much the cause of failure as the former:

Improper storage—brick should be kept dry and not allowed to get damp and freeze;

Unsuitable mortar—it should be, as Mr. Unger says, of the same character as the brick;

Poor mason work is one of the commonest causes of failure. My old mason foreman would not put a man on fire-brick work unless he could cut stone, saying "I want masons—the others are only brick layers;" **Mr. Stone.**

Poor furnace design. This is generally due to the inexperience of actual work of the man who designed details. No man should design any part of a furnace until he has sat up a good many nights with one. Too rapid drying and heating of a furnace also frequently cause trouble.

MR. UNGER.—I am glad that Mr. Stone has brought up the question of the sizing of the material. We have been studying this in its application to silica brick, and believe it is one of the most important questions in brick manufacture to-day. One of the sub-committees of Committee C-8 on Refractories is making a special study of this question. **Mr. Unger.**

MR. NOLAN.—It has just been said that silica bricks have virtually replaced fire-bricks. If it is a proper question, I should like to ask what are the relative costs of the two materials at the present time. **Mr. Nolan.**

MR. UNGER.—Just about the same. **Mr. Unger.**

OPTICAL PROPERTIES AND THEORY OF COLOR OF PIGMENTS AND PAINTS.

By H. E. Merwin.

SUMMARY.

The hue, purity and brightness of light diffused by a pigment or paint depends upon the refractive index, color absorption, size, shape and texture of the pigment grains, and upon the refractive index, color, and continuity of the vehicle; and also upon the distribution of the grains in the vehicle.

A black pigment to be most effective optically should have (1) a refractive index equal to that of the surrounding medium, and (2) grains 1 μ (or less) in diameter which should be (3) just barely opaque to all colors.[1] All the ordinary black pigments possess the third characteristic approximately, some of them the second, but only one of them, bone black in its various forms, has approximately the best refractive index.

A white pigment should have a high refractive index, should be even grained, with grains about 0.5 to 1.0 μ in diameter. Finer grains diffuse blue light more than red light under certain conditions, for example, when mixed in oil with black or dark red pigments. Thus blue grays and purples result. Most white pigments contain enough very fine grains to give bluish grays. Control of size of grain is important. It is probable that the refractive index of certain zinc oxides made by the American process can be controlled advantageously.

Coloring efficiency must be considered as much from the standpoint of the hue and tone desired as of the absolute "quantity of color" obtained. Thus, although extremely fine division may favor the last factor mentioned, when the pigment

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[1] The term μ is the symbol for a micron (0.001=mm.).

is used in a mixture with a strongly diffusing pigment, yet under other conditions size and shape of grain and refractive index are of great importance. For example, Harrison red in grains of one shape has about 10 per cent better diffusing power than in grains of another shape. The shifting of hue, especially of orange and yellow paints toward the green, due to admixed black is considered in detail. The optical properties which determine whether a pigment is best suited for producing tints or shades are discussed.

Special methods of studying the optical properties of pigments have been used, and a considerable number of optical constants determined. These properties can be applied in determinative work or to problems in chromatics.

This investigation has been conducted in the Geophysical Laboratory of the Carnegie Institution of Washington.

OPTICAL PROPERTIES AND THEORY OF COLOR OF PIGMENTS AND PAINTS.[1]

By H. E. Merwin.

I. Introduction.

Color is commonly described in terms of hue, purity and brightness, which respectively characterize color with regard to the position of its equivalent or complementary in the spectrum; the amount of admixed white light; its brilliancy compared with a standard white. Another term to designate a pure hue of maximum brightness is desirable. The term "full color," which has been used in an indefinite way, will be used here in this definite sense.[2] The brightness of a paint may be decreased by admixture of a black pigment. For present purposes the color of a pigment or paint is known adequately if the percentages of full color, white, and black are given.

Ridgway's[3] color standards are constructed on a theoretical basis similar to that just outlined. The standards themselves are not of theoretical purity or fullness, but they are excellent for comparative tests, and have proved satisfactory in the experimental work later described. All the direct comparisons of color herein are based upon them.

II. General Optical Relationships of Pigments.

Under standard conditions of observation the optical properties of a painted surface depend upon the pigment, the vehicle and the surface covered. Here, surfaces will be considered only incidentally; vehicles will be considered in relation to the optical phenomena of pigments. Attention will be directed chiefly to pigments.

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[1] This study was suggested by P. H. Walker, chairman of Committee D-1 of the Society, who furnished the writer with many of the pigments examined.

See a short paper by Wilhelm Ostwald, ***Kolloid Zeitschr.***, Vol. 16, pp. 1–4 (1915).

[2] A pure hue, which is equivalent to a single wave (+ 1) length, can be produced by combining light of a series of wave (− 1) lengths on one side of the equivalent wave (− 1) length with another series on the other side. A full color results when a maximum amount of such light is combined. Wave (+ 1) lengths beyond the range of these two series act as complementary to some of those present, and thus introduce white. The lightness of some yellows—barium yellow, for example—is due in part to the presence of the complementaries blue-green and red.

[3] Robert Ridgway, "Color Standards and Color Nomenclature," Washington, D. C., 1912.

A property common to all pigments and painted surfaces, that of diffusing light, must be considered in almost every problem concerning the optical properties of paints and pigments. This property is a complex result of certain fundamental relations of properties of pigment grains and surrounding vehicle, or other medium. Grains must be considered with respect to refractive index, color absorption,[1] size, shape, and texture; dried vehicles with respect to refractive index, color absorption and quality of surface. The amount of light reflected from a unit area of surface of a grain or of a paint vehicle increases with the refractive index and absorbing power. The effective reflecting surface of a given volume of pigment increases with the fineness of grain until the grains become too small to act as regular reflectors, that is, when their diameter is about the length of a wave of light in the medium containing the pigment. Smaller grains scatter light, but scattering is not as efficient as reflection. (See Section X.)

III. OPTICAL RELATIONS OF VEHICLE TO PIGMENT.

Dried oil, varnish and albumen have refractive indices of about 1.5, and smooth surfaces reflect about 4 per cent of the light which falls perpendicularly upon them. Minutely rough, "flat" surfaces produced by drying would reflect about the same amount, even if the effects of the enclosed pigment and underlying surface were eliminated. Thus, the "blackness" of a black paint is limited by the reflecting power of the vehicle. Pigment grains surrounded by a vehicle reflect less than in air,[2] because the ratio of refractive index of grain to surrounding medium is less. The most general consequence of this is to cause light which enters the surface of a film of paint to traverse more pigment grains before it returns through the surface, than it would traverse if the pigment grains were surrounded by air.

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[1] The purpose of the following sections is to indicate essentially the significance of certain other factors than color absorption which enter into the problems of color of paints. Rood ("Modern Chromatics") devised several experiments to show effects of some of these factors, but some of his interpretations need revision. Color is discussed from many view-points, and lists of reference are included in "Color and its Applications," by M. Luckiesh, 1915.

[2] There are possible exceptions in the case of pigments with metallic reflection.

Thus, more light may be absorbed and the paint appear darker than the dry pigment. (See Sections IV and XV.) The wave-length of a given color of light is less in paint vehicles than in air. Thus, grains of a given size scatter light differently in these different media. (See Section X.) The use of colored vehicles and glazing colors is discussed later.

IV. REFLECTION, AND PRIMARY AND SECONDARY DIFFUSION OF LIGHT BY PIGMENTS.

Light striking a pigment grain is partly reflected or scattered without traversing the grain, is partly absorbed and partly transmitted to the far side of the grain where some emerges and some is reflected within the grain. Several grains in succession may take part in turning back into the air what is left of light which had penetrated a mass of paint or pigment. Light which has been returned into the air without traversing any pigment grains will be called primarily diffused light; light which has traversed grains before being returned will be called secondarily diffused light. From pigments with metallic luster—opaque pigments such as aluminum, gilt, bronze and graphite—light is primarily diffused; from ordinary transparent pigments light is diffused in both ways, but in greater part secondarily.

V. COLOR ABSORPTION AND SIZE OF GRAIN.

An adequate discussion of the colors of pigments must be based upon knowledge of the color absorption of single pigment grains. Most pigments, except lakes, consist of minute crystals. Variation of color with direction in crystals is common (pleochroism). Therefore, the shape of crystals may determine their value as constituents of a pigment. For example, one factor in the variation in color of red leads is the shape of the crystals with respect to their color axes. Also, "permanent" violet would have much more coloring power if its crystals were flattened in a different direction.

The boundary in the spectrum between wave-lengths (spectral hues) that are strongly absorbed, and those that are freely transmitted by a pigment substance, is always so wide that small grains transmit considerable light that would be absorbed by large grains.[1] A pigment consisting of small grains tends to be brighter and have a hue represented by a wave-length nearer an absorption band, than a pigment consisting of large grains.[2] (Fig. 7.)

Two extreme types of light transmission deserve consideration. In one, large grains are highly transparent to hues which characterize their color; in the other, even the characteristic hues are largely absorbed by small grains. Examples of the first are chrome yellows, red lead, cobalt blue and emerald green; of the second, ferric oxide and chromic oxide.

Coloring power increases as size of grain decreases, but at very different rates for different substances.[3] The rate for a given substance depends upon the method of diffusing the color and upon the extent of dilution of the color. Diffusion may be brought about by the surface over which the paint is spread (glazing colors), or by admixed grains having diffusing power, or by the pigment grains themselves. Maximum coloring power cannot be expected if single grains—or two superposed grains in case of intermediate hues of mixtures—are more deeply colored than the final diffused color sought. On the other hand there is little advantage in having grains much finer than this unless the diffusing power improves with fineness. From this it would appear that two samples of the same pigment may have different coloring power when mixed with a large excess of white or when used as glazing color, and yet not be appreciably different when used in making nearly full colors or shades.

If the grain size of a secondarily diffusing colored pigment could be gradually decreased below the optimum, the resulting paint would become first a turbid medium and then it would clear until it appeared like a colored solution. There is no good evidence that marked changes in hue in a particular direction would take place during the process. In the latter condition it would possess the maximum coloring power if used as a glazing color.

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[1] In cases where the terms large, small, etc., have needed more definite limitation about the following limits have been in mind: very small, less than 0.8 μ, small, 0.8 to 2 μ; medium, 2 to 5 μ; large, 5 to 10 μ; very large, greater than 10 μ.

[2] Most colored pigments do not contain significant amounts of separate grains smaller than 0.5 μ or larger than 10 μ in mean diameter. Prussian blue is often exceptionally fine grained, and emerald green, verdigris, true cobalt blue, some lakes and some natural pigments are exceptionally coarse grained.

[3] For a given amount of pigment the amount of surface exposed to intercept light increases inversely as the square of the diameter of the grains, whereas the power of a grain to modify transmitted light in any direction increases more slowly than the diameter. A crystal of potassium chromate or of copper sulfate 1 mm. in thickness has about as much depth of color as one 5 mm. thick. Pigments of the first type above mentioned are less affected by changes of grain than the second.

Somewhat at the expense of coloring efficiency the hue of a paint can be slightly controlled by controlling the size of grain. Grains of an even size are better, because larger grains absorb excessively. Basic lead chromate in medium-size grains is scarlet, but in medium to small grains it is nearly orange. (See Section XVII.)

With regard to the adaptability of colored pigments to the making of tints and shades, four classes of pigments may be considered. (See Sections II, XIII and XIV.)

1. Colored grains are chiefly of such size that if closely packed in a single layer they would transmit (or diffuse and transmit) a clear tint (say roughly 40 to 60 per cent white). From 5 to 20 such layers would produce a full color. Either clear tints or pure shades can be made from such a pigment. Examples: chrome orange, chrome yellow, verdigris, ultramarine blue.

2. Grains are so transparent that white light after traversing many layers of grains still contains a good deal (20 per cent or more) of white. Such a pigment can be used in making clear tints, but not pure shades. Examples: barium yellow, basic copper carbonate, strontium yellow.

3. A single layer of grains absorbs several per cent of the characteristic hue, and other hues almost completely. Pure shades and dull tints can be made from such a pigment. Examples: vermilion, scarlet chromate, Harrison red, chrome green.

4. Single grains absorb several per cent of the characteristic hue, and even several layers of grains do not absorb other hues completely. When darkened by a black pigment dull shades result, and when lightened by a white pigment dull tints are formed. Examples: Naples yellow, some Dutch pinks and yellow ochres.

In the last two classes diffusing power determines to some extent, and absorbing power to a greater extent, what range of pure shades can be obtained.

A distinction between tint and shade when both black and white modify a hue can not be made on any generally accepted basis. M. Luckiesh[1] favors the idea—which appears most acceptable—that a shade results from diminishing the brightness of the standard hue by means of either gray or black. On this basis a tint would result from increasing the brightness by means of white, or a gray which is brighter than the standard hue. Ridgway selected a definite gray, and considered that lighter grays produce tints and darker grays shades.

VI. RELATION OF COLOR TO REFRACTIVE INDEX.

To be most effective as a pigment when used alone, a substance should have a high refractive index for the color which it most freely transmits. In general there are large variations of refractive index near and through a region of color absorption. Refractive index is higher on the long-wave side of such a region than on the short-wave side. For this reason red, orange, and yellow pigments usually have much higher refractive indices than blue and violet pigments. The refractive index of lakes is largely determined by the base, and is always comparatively low. Some pigments so nearly match vehicles in refractive index that they diffuse very little light. They become effective only when mixed with a pigment of high refractive index which will diffuse their color, or when painted in thin films over a surface covered with a strongly diffusing paint. For example, Prussian blue, verdigris, and alizarine lakes. (See Sections XII and XVII.)

VII. BIREFRINGENCE AND OPTICAL ORIENTATION.

The refractive index of anisotropic crystals varies with direction—birefringence. If birefringence is strong the shape of the crystal may greatly affect its efficiency as a diffuser of light in a mass of pigment. A very noteworthy example is Harrison red. (See Section XVII.) With the refractive index axes of prismatic grains in the most effective positions the diffusing power is about 10 per cent more than with the axes in the least effective positions.

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[1] "Color and its Applications," New York, 1915, p. 71.

VIII. HIDING POWER.

The standards of the Society define hiding power as the power of a paint or paint material as used to obscure a surface painted with it.

The foregoing considerations indicate the several factors here concerned. Hiding power thus defined increases with increase of the following properties of the pigment: refractive index, fineness (till optimum size is reached), depth of color, and in certain cases with the roughness and complexity of the pigment grains. The hiding power of a paint increases with increase of hiding power of the pigment, with decreasing refractive index of the vehicle, and with increasing depth of color of the vehicle. Mention should be made of slight but indefinable variations due to irregularities of the surface of the paint film, and to the distribution of the pigment[1] throughout the film. It seems probable that the lack of agreement among observers who have compared the hiding power of white pigments is due in part to differences unprovided for in the grain of the pigments.[2]

IX. THE BLACK PIGMENTS.

A pigment to be really black should have a refractive index the same as the dried vehicle in which it is used, and should have such absorptive power for all colors as would cause incident light to be about all absorbed by particles 1 to 2 μ in diameter. Such absorption power would not give the pigment as held in its vehicle any significant reflecting power. In the dry state such a pigment would reflect a little light, and therefore be a very dark neutral gray. A compact film of paint made from such a pigment would not be absolutely black, for the surface of the vehicle would reflect about 4 per cent of light. A film in which the pigment is held loosely, such as is produced by using a little binding material with a large amount of volatile vehicle, may give a diffuse reflecting power as low as about 0.5 per cent.

Ivory black is very nearly a perfect black. Its refractive index is only slightly greater than oil or varnish, and its absorptive power is such that all except a small percentage of its grains are nearly opaque. The sight color transmitted by a single grain is brownish, so that when these grains are mixed with a coarse-grain white (see Section X) a gray is produced that is perceptibly brownish. When used alone, the brown color transmitted by the first grains traversed is absorbed by succeeding grains so that a most intense black results. A thick film of pure Prussian blue, or even ultramarine, in oil is black. In this case the blue is more freely transmitted than the brown of ivory black, but the refractive index of the pigment is so near that of the oil that the blue light is not diffused outward.

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[1] The pigment grains nearest to the exposed surface of the film are most effective Compare the effect produced by the ground side of a glass against a surface, with the effect when the glass is reversed.

[2] Tests on opacity and hiding power of some white pigments have been made by G. W. Thompson, *Journal of Industrial and Engineering Chemistry*, Vol. 5, p. 120 (1913).

The black residue left after extracting the calcium salts from ivory (or bone) black cannot be definitely characterized, but the shrunken and disintegrated grains still have about the same refractive index and absorptive power as ivory black, but they are less dense.

Graphite is not an ideal black pigment because of its high reflecting power. The color of its reflected light[1] is faintly brownish. Its absorption is so strong that the light transmitted by pigment grains is entirely negligible. Repeated reflections within a film of graphite paint cut down the intensity of the diffused light, so that such a film appears very dark neutral gray.

Lampblack and similar blacks are typically composed of practically opaque ovoidal particles about 1 μ in diameter adhering loosely in strings or clusters. The reflecting powers of these materials are probably not greatly inferior to those of graphite, for when they are compressed strongly against a polished steel surface they reflect well. Their fine state of division gives them great tinting strength.

The sample of magnetic-oxide black which was examined consisted of spheroidal particles, mostly between 0.8 and 1.5 μ in diameter. These were appreciably transparent in dark to light browns. The refractive index is probably near that of graphite.

Some of the optical effects produced by one of these blacks may not be produced by another. (See Section XI.)

X. THE WHITE PIGMENTS.[2]

The most important characteristics contributing to the efficiency of a white pigment are non-absorbing power, high refractive index, weak optical dispersion, fineness and evenness of grain. If optical dispersion is strong, the reflecting power for blue and violet light may be so much greater than for red as to give the pigment a bluish color under certain conditions. There is a critical size of grain for each homogeneous white pigment, depending upon the optical properties of the grains and vehicle and the distribution of the pigment grains. This size is from about 0.5 to 1.0 μ. Larger grains have too little reflecting surface per volume of substance, and grains much smaller do not act as regular reflectors at all unless very close together, but they scatter light. Grains having a diameter close to a wave-length of green light in oil—0.35 μ—may reflect violet more strongly than red. Neither scattering, nor the differential reflection just referred to, have significant effect on the tint of a white pigment, provided the grains are close together and in a thick film, as they are on a well-painted surface. For, although the red penetrates deeper, it is finally scattered or reflected to practically the same degree as the blue. But if the film is thin or the particles are widely spaced, considerably more red than blue finds its way through the film. Suppose such a film covers a black surface, then the excess of red is absorbed by the surface and the film-covered surface has a blue-gray appearance.[1] A white pigment containing no grains less than 2 μ in diameter under the same conditions produces a neutral gray. In certain mixed pigments these effects of size of grain are very pronounced. (See Sections XI and XIV.)

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[1] When compared with a neutral gray in a reflecting microscope.

[2] For details, see Section XVII. A paper by H. A. Gardner is in most respects in accord with this study; *Journal of Industrial and Engineering Chemistry*, Vol. 8, p. 794 (1916.)

Samples of the various white pigments were tested over a black surface. All of them gave more or less bluish grays, dependent upon the amount of very fine grains which they contained. Search was then made amongst a large assortment of chemicals—in the form of white powders—to find one which would produce over black a true neutral gray, and which would also be a fairly "strong" white. A sample of lead sulfate was found satisfactory. Its grains ranged between 2 and 5 μ. This sample may be called a "true" white.

Zinc oxide has a high refractive index and absorbs no color except in the extreme violet. It can be produced in clear grains of various sizes by the dry processes. The burning vapor of metallic zinc produces an exceedingly fine-grain oxide—grains much less than 1 μ in diameter. Reheating causes growth of some particles at the expense of others, with the ultimate development of a definitely crystallin powder.

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[1] A blue of considerable brilliancy (blue 25 parts, neutral gray 75 parts) can be produced by pressing a film of very fine-grain zinc oxide paste very thin, under a cover glass on a glass slip blackened on the under side.

The burning of zinc blende produces a powdery oxide less even-grained than that from metallic zinc. If the blende contains a few per cent of lead the pigment after reheating may contain a large proportion of particles more than 2 μ in diameter, lacking definite crystallinity, and low in refraction.

Most of the zinc-oxide pastes contain enough material finer than 1 μ to add considerable blue color, due to scattering somewhat, to paints in which they are mixed, and to reduce the hiding power of the paints.

White lead is a definitely crystallin material even less light-absorbing than zinc oxide, and of about the same refractive index. That made by the Dutch process is considerably coarser than zinc oxide. Greater hiding power would be secured from a finer-grain powder, such as may be produced by precipitation.

Basic-sulfate white lead has lower refraction than white lead or zinc oxide, and is about as fine as zinc oxide.

Lithopone contains grains of various refractive indices and sizes. Reference to the detailed description of a sample is necessary to the understanding of its properties.

XI. EFFECTS OF MIXING BLACK AND WHITE PIGMENTS.

The true white previously described (see Section X) was mixed with several black pigments to produce grays. Graphite and lampblack produced neutral grays, but ivory black and magnetic-oxide black produced slightly brownish (orange) grays. The finest-grain French-process zinc oxides, with graphite and lampblack, produced very blue grays; some contained as much as 10 per cent blue. American-process zinc oxides and other commercial white pigments gave grays containing 2 to 5 per cent blue. Thus, in mixture with black, these fine-grain whites give blue grays which are, however, less blue than may be produced over a background of black. (See Section X.) The orange tinge of grains of ivory black and magnetic-oxide black may neutralize the blue which white pigments produce in mixtures, so that neutral grays can be made. A test was made to determine how much blue would be required to be mixed with a true white, to give with ivory black a blue-gray like that produced by graphite and White-Seal zinc oxide. The blue pigments used were ultramarine, Prussian blue, and true cobalt blue. It was found that a bright blue, corresponding to 70 to 80 per cent blue, was required.

XII. COLORED PIGMENTS IN MIXTURES.

Only in the form of pastes are mixtures considered in this section. The colors of the same or similar pigments mixed dry are discussed in Section XV.

Intermediate Hues.—So far as color absorption alone, without regard to refractive index and size of grain, is concerned, the theory of the production of intermediate hues is well known. In these cases transmitted light only has been considered. For example, a thin film of Prussian-blue paint is blue by transmitted light; likewise, a film containing powdered sodium chromate is yellow. Green is transmitted by the two films superposed or a film made by mixing. But a film of the mixture over a black background scarcely reflects any light at all, except the small amount from the surface of the vehicle. If lemon chrome, which in dry powder is nearly the same yellow as sodium chromate, is substituted for the sodium chromate, then a fairly bright green is diffusely reflected, and much less transmitted. The lemon chrome, because of its high refractive index, reflects diffusely the green light transmitted by superposed grains of the two pigments in the mixture. Hydrous sodium chromate and Prussian blue, on the other hand, have nearly the refractive index of the vehicle. The quality of the green produced by such mixing depends upon all the fundamental optical properties of the pigment grains. In the usual case, the intermediate hue is considerably absorbed by both substances, thus rendering impossible the production of a very bright color, and making fineness of grain essential. If the grains of the substance having highest refraction are of optimum size for diffusing light, then the other substance may profitably be much finer, for example, lemon chrome yellow with very fine Prussian blue. Or it may be best to have both pigments very fine and accomplish the diffusion by admixing an effective white pigment. In some cases the colored substances form very thin films upon suitably sized grains of either "strong" or "weak" white pigments.[1] In other cases films of one pigment are precipitated upon the other.[2] This last is a most efficient method of using the precipitated substance, but only such substances as are very strongly colored can be thus used.

In case both substances are highly transparent to wavelengths producing the intermediate hue, the size of grain is of little importance in the production of a full hue, but it is of the same importance as usual in relation to diffusing power. For example, a green of great intensity can be obtained from lemon chrome of usual grain and bluish verdigris, although the verdigris is coarse grained.

In any case, grains near the exposed surface of the paint film will reflect some white light and some light which has traversed grains of only one color. The result is a hue of slightly decreased purity.

Purple is considered an intermediate hue between red and violet, in the sense that if pigment grains transmit and diffuse both red and violet or red and blue, the resulting color is purple in proper proportions. Very few substances suitable for pigments possess such transparency except in a slight degree. Therefore, only dark purples are produced by mixing two pigments, one of which is predominantly red and the other predominantly violet or blue. But by adding a strong white to such a mixture the hue can be considerably intensified. (See Section XIV.)

Mention has already been made of the scattered blue produced by mixing a very fine-grain white with black or other dark pigment. This blue can be made available in the production of purple when certain reds are used.

XIII. HUES MODIFIED BY ADMIXTURE OF BLACK.

A Simple Wet Pigment Mixed with Black.—The chief effect of mixing black with a pigment having one constituent, is to decrease the brightness by decreasing both the white and the colored components of the diffused light, but marked changes in hue may result. Hue changes are chiefly the result of three tendencies, namely, migration of the hue toward (1) the violet end of the spectrum, (2) the absorption band, and (3) the color most refracted. The combined effects of these tendencies are shown in Fig. 7.

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[1] Some of the most brilliant chrome greens.

[2] Para red upon red lead.

1. All pigments reflect diffusely colors representing a considerable range of wave-lengths. Admixture of black diminishes the physical intensity of the whole range of wave-lengths to the same degree, yet the apparent (physiological) intensity of longer wave-lengths is diminished more than that of shorter wave-lengths. Thus the resulting color differs from the original in hue as well as in brightness.[1] The effect is most noticeable in the part of the spectrum, yellow, for which the sensibility of the eye to differences in hue is greatest. A yellow pigment greatly darkened tends to appear dark greenish yellow, and a darkened yellowish green tends to appear bluish green.

2. Black grains interposed among colored grains intercept light which would otherwise traverse more colored grains, before being returned through the surface of the mass. Thus the emergent light is less completely robbed of the wave lengths near the absorption band, than if the black grains were not present. The tendency toward change of hue is most pronounced in fine-grain pigments, especially if absorption increases gradually toward one end only of the spectrum.[2] For example, chrome yellow absorbs gradually through the blue and violet, so that when mixed with black it becomes more greenish than the Purtinje effect alone would account for.

3. High refraction favors diffuse reflection; therefore, that component of color for which pigment grains have the highest refractive index is diffusely reflected out of the mass of grains, after traversing a shorter average path than that component which is less refracted. Thus, fewer admixed black grains will

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[1] Besides this (the Purtinje effect) there is probably another physiological effect even more pronounced which has led to such color names as olive-green, calla green, etc. These are really only dark yellows (Ridgway, Plates IV, V).

If the spectra from a diffraction grating are observed through a Wratten filter No. 77—or 77 and 58—which absorbs a narrow band in the middle of the yellow, the brightest spectra show bright yellow near the absorption band grading into green. But in the fainter spectra green encroaches upon the yellow so that in the faintest spectra only green is seen.

Four sets of yellow screens were matched with spectral colors by means of two microscopes with rotating nicols and a comparison eye-piece. After darkening about 95 per cent no change of hue of the screens of more than 2 *μμ* toward the green was observed. Thus the Purtinje effect in Ridgway's colors seems to be negligible.

[2] **Two absorption bands complicate the application of the principle, but do not affect it otherwise.**

be encountered by the former, and it will be correspondingly less diminished in intensity. This effect is small compared with the two just described, and is negligible in most cases.

Mixed Pigment plus Black.—In addition to the foregoing tendencies another of importance must be considered. If a paint contains two pigments—in separate grains—which differ markedly in hue, then adding black will cause the brighter hue to tend to dominate. For example, orange made from yellow and red will when darkened with black tend to become like a darkened yellow. This tendency may be about balanced by the tendency of high refraction to favor diffuse reflection, as when zinc yellow and verdigris are mixed with black.

XIV. COLOR MODIFIED BY ADMIXTURE OF WHITE PIGMENT.

Adding white increases the brightness of a paint, but there may be notable changes in hue also.

The chief brightening effects (see Fig. 3) can be discussed best if the original paint is assumed to contain but one pigment. If the paint is a tint or nearly full color, the resulting color will be a tint having intermediate brightness. If the paint is a dark or intermediate shade then the diffusing power and absorption (or transmission) characteristics of the pigment must be considered.

Weak Diffusing Power.—If the transmission is high, the first small portions of white added diffuse the color and thus increase the percentage of color to a high maximum; further additions of white produce tints intermediate between the maximum color and white. Examples: verdigris, rose madder and ultramarine. If the transmission is moderate or low the effects are similar, except that the maximum color is duller. Examples: burnt sienna, Dutch pink, permanent violet.

Strong Diffusing Power.—If transmission is high, even the first portions of white added will dilute the color. If transmission is moderate or low, the first white added will tend to increase the color but the amount of such increase will not be large, especially if the pigment does not contain small grains. Examples: vermilion, chromic oxide, Indian red, bright red oxide, Venetian red. (See Fig. 3.)

The hue changes produced by adding white to a colored one-pigment paint depend upon the fineness of the white, as well as upon the properties of the colored pigment. The blue and violet light scattered and differentially reflected under certain conditions by fine-grain white pigments produces corresponding changes in hue when mixed with the color from a colored pigment. Although there are many conditions under which this blue appears,[1] it is striking in case of colored pigments only when the original color is some tone of red, preferably a dark red shade produced by a pigment having strong primary diffusing power. In this case the blue, which is a considerable fraction of the total light diffused, combined with the red gives purple. Indian red with White-Seal zinc oxide gives a decided purple, while with a coarse-grain white the lightened color inclines toward orange. Even light tints of alizarin lake, vermilion and rose madder are quite noticeably different when made with the whites just mentioned. (See Fig. 7.)

Strongly diffusing white grains among colored grains cause a decrease in the distance which light traverses through the colored grains; thus there is a tendency for the hue of a colored paint to migrate toward the absorption band of the pigment when white is added. (See Fig. 7.) The amount and rate of the migration compared with the brightening depend chiefly upon the fineness and the transmitting properties of the colored pigment. In general the tendency is slight for coarse-grain pigments. A very fine-grain pigment having moderate transparency, such as Prussian blue, will show decided migration. One with fine grain, high transmission and a very indefinite absorption boundary will show a large migration; for example, crystallin lemon chrome yellow. Zinc chromate and chrome yellow when not crystallin may be so coarse grained as not to show appreciable change of hue when tinted out.

Mixed Pigment.—In general the hue of a mixed pigment or paint cannot be so greatly intensified (brightened) by the addition of white as the hue of a simple pigment; otherwise the brightening effects of added white are essentially the same in the two cases.

The migration of the hue of a mixed paint caused by adding a true white depends upon the position of the absorption bands of the colored pigments. If the absorption bands are on opposite sides of the hue, as in green and purple, the tendencies to migrate toward the two bands will be more or less balanced. If the bands are on the same side of the hue, as when orange is produced by mixing red and yellow, the tendency to migrate toward the absorption band will be greater than for either pigment alone. Some red pigments transmit enough violet to affect appreciably the migration of hue of a mixed orange. An uneven-grain pigment with a very gradual absorption gradient will behave like the mixed red and yellow.

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[1] See Section X for theory.

XV. DIFFERENCES IN COLOR BETWEEN WET AND DRY COLORED PIGMENTS.

The difference in refractive index between air and paint vehicles is so great that the diffusing power of a pigment is much less wet than dry. On account of high primary diffusing power (see Section IV) all dry colored pigments diffuse notable amounts of white light. For this reason among others, their colors are impure. Wetting greatly decreases both primary and secondary diffusing power, and thus increases the purity of color and decreases its brightness. Pigments having about the refractive index of the paint vehicle are more greatly affected than those having high refractive index. Decrease in brightness may be accompanied by an increase or decrease in the percentage of color (hue), and also by displacement of the hue. Size and constants of absorption as well as refractive index of the component grains must be considered in a further discussion of this topic.

One Constituent.—In the case of a simple pigment, wetting causes the hue to be displaced away from the absorption band because the lessened diffusive power permits deeper penetration and greater absorption. (See Section V.)

Factors which favor great increase in the percentage of color on wetting are moderate to weak absorption, fine grain, moderate to high refractive index. Such a pigment when dry will give a bright tint, when wet it will still have sufficient secondary diffusing power to prevent much absorption of the characteristic hue. Other favorable factors remaining the same, it is evident that there is an optimum refractive index, which is probably less than 2.0 and greater than 1.75. It is essential that absorption be not strong. A number of colored substances showing such change are shown in Fig. 5.

Low refractive index, strong absorption, and coarse grain favor a decrease in the percentage of color on wetting, but the effect is shown even with fine-grain highly refracting substances if absorption is excessive, for example, Indian red.

Changes in brightness due to wetting depend chiefly upon the relation between the absorption gradient and the diffusing power. High refractive index and optimum fineness of grain are necessary to prevent large loss of brightness when a pigment having a gentle absorption gradient is wet. On the other hand if the absorption gradient is steep, loss of brightness on wetting will be far less dependent upon size of grain and refractive index. Examples of pigments in which such change is small are zinc yellow, lemon chrome yellow, red lead, and emerald green.

Migration of hue caused by wetting simple pigments is usually slight. Low refraction, fineness of grain and gentle absorption gradient favor such change.

Mixed Pigment.—The darkening and change in intensity of the colored component of a mixed pigment caused by wetting can be largely inferred from what has preceded. The migration of hue, which may be large, needs a little more consideration. Red and yellow mixed dry produce a somewhat dull orange. Wetting causes a pronounced displacement of the hue toward red. A large part of the yellow green diffused by the dry yellow pigment is absorbed by the red pigment when deeper penetration is permitted by the wetting. In case of a mixed green, wetting causes about equally increased absorption of yellow and blue.

Red and blue mixed wet produce so dark a purple that its hue can scarcely be determined. The addition of a little white increases the color. The mixture when dry has in general a different hue, owing chiefly to the tendency for the hue to be displaced toward that of the more strongly diffusing colored pigment.

A very notable effect of strong secondary diffusion, especially in dry mixed pigments, is color due to juxtaposition of colored grains at the surface of the pigment mass. Each grain diffuses light of its own color besides light which may have traversed grains of another color. Much of the purple produced from red and blue originates thus. A neutral gray diffusing more than 25 per cent of white can be made from dry red, green and blue pigments.

XVI. METHODS.

Some pigments contain many composite grains which do not become disaggregated when spread in a film of paint. Such grains vary from compact masses to masses which consist of nearly spherical component grains joined so as to have a large amount of pore space. The term "size of grain" when grains are porous might better take definite account of size of single grains and size of composite grains, and number or volume of each. Fresh oil pastes should be used when possible in testing grain, for these contain the pigment in as nearly as possible the same state of aggregation that it will have when spread. Of course some pigments can be mounted on a microscope slide in practically the same state of fineness whether dry or wet, but others can not.

Mounting for microscope study should not be done in a mobile liquid, for the finer grains tend to concentrate near one edge of the cover glass, and even loose aggregates from a dry pigment are broken up in it with great difficulty. A very viscous, sticky mounting material can be made by melting together about equal parts of rosin and camphor. The viscosity of this mixture can be lowered greatly by slight heating. Thick Canada balsam may be used less satisfactorily. The object of mounting is to obtain a film as thin as the largest pigment grains will permit, with the grains separated sufficiently for counting. This can be accomplished by mixing a very little of the mounting material and pigment under a small cover glass which is given a rotary and sliding motion by firm pressure. There is little possibility of crushing grains by this means.

Measurements of optical constants were made by means of the petrographic microscope and standard refractive immersion media.

XVII. OPTICAL PROPERTIES IN DETAIL.

Graphite.—(See Section IX.) Graphite cleaves readily and forms in plates parallel to the cleavage, so that the optical properties measurable on cleavage flakes are chiefly responsible for its behavior as a pigment. Microscopic flakes thin enough to transmit grayish light have been prepared. Its refractive index ω is about 3, and its reflecting power high (about 37 per cent).[1]

Ivory Black, Bone Black, Drop Black, etc.—(See Section IX.) In charred bones a small amount of dark carbonaceous matter is held in the most minute subdivision throughout a large amount of calcium phosphate. Grains 5 μ in diameter transmit an appreciable brownish color and appear practically homogeneous optically. Their apparent refractive index, when their pores are filled with oil, ranges from about 1.65 to 1.70. Thus, the desirable characteristics of slight diffusing power and effective absorbing power are combined.

Basic-Carbonate White Lead.—Basic-carbonate white lead is a definitely crystallized substance. Individual grains seen in several samples were tabular (perhaps twice as broad as thick), and hexagonal in general outline.[2] The two chief refractive indices[3] are $\omega = 2.09$, $\epsilon = 1.94$. In ground samples of this pigment relatively few grains, except possibly the smaller, are single crystals; most are made up of several small crystals or fragments firmly intergrown in various orientations. Such grains are more effective diffusers of light than those of equal size which are single crystals. About 40 per cent by calculated volume of each of four samples studied, consists of grains less than 2 μ in diameter and about 90 per cent less than 5 μ. Several per cent of the "normal" lead carbonate is often present in commercial white leads. It is easily identifiable microscopically, because it occurs in clear crystal fragments of relatively large size which, as usually oriented, have much stronger double refraction than the basic carbonate. Also, its least index of refraction is only 1.80, and it has a strong tendency to appear in negatively elongated prismatic grains. Tables of the basic carbonate seen edgewise have positive elongation. The highest refractive index of the normal carbonate is 2.08.

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[1] Wartemberg, *Ber. dent. phys. Ges.*, Vol. 12, p. 105 (1910); also Zakrzewski, *Anz. Akad. Miss. Krak.* 1910, p. 122.

[2] These crystals when studied more minutely were seen to be a rhombohedron and base. From some of them a clear uniaxial negative interference figure was obtained. In these respects they correspond to the mineral hydrocerusite. L. F. Hawley, *Journal of Physical Chemistry*, Vol. 10, p. 654 (1906), proved that white lead is a definite compound by a study of its solubility The formula thus obtained agrees with hydrocerusite.

[3] From the natural mineral, hydrocerusite, E. S. Larsen obtained $\omega = 2.07$ (unpublished).

Basic-Sulfate White Lead.—Basic-sulfate white lead is not definitely crystallin. It consists of minute spherules which are themselves firm aggregates of much more minute particles. The single refractive index of the sample studied was 1.93. It is probable that samples will differ slightly in refraction. From the structure of the grains it is inferred that they have a much rougher surface than the crystal grains of zinc white. Yet owing to the minuteness of the grains, this roughness probably does not very materially affect the diffusive power of the pigment. This sample was almost identical in fineness with white zinc, but it contained larger, loosely aggregated composite grains. Such grains, however, must have nearly as great diffusive power as if distributed singly.

Lithopone.—The single sample of lithopone studied did not contain identifiable crystals although there was doubly refracting material present. Pronounced lack of homogeneity was brought out by differences of refraction in single grains and among different grains. Doubtless such lack of homogeneity extends to the sub-microscopic magnitudes. The term "grain" here used means, then, fragments produced by pulverizing firmly aggregated material. In this sample about 70 per cent, by calculated volume, of the grains were smaller than 2 μ. Only a small percentage were larger than 4 μ or smaller than 0.6 μ. Few grains larger than 1.5 μ had uniformity of refraction. Portions of the sample were examined in liquids of measured refractive indices, and the indicated percentages by volume of the sample which were higher in refraction than the liquid were estimated roughly as follows: 1.70, 99 per cent; 1 74, 97 per cent; 1.78, 85 per cent; 1.84, 65 per cent; 1.90, 20 per cent; 2.10, 5 per cent; 2.25, 1 per cent.

Lithopone is prepared by heating and quenching a simultaneously precipitated mixture of zinc sulfide and barium sulfate—a process which might conceivably give so intimate a mixture that it would appear microscopically homogeneous. But if consideration is given to the very great difference between these two substances in tendency to crystallize from aqueous solution, then the graded heterogeneity observed may be accounted for in something like the following way: Within the mass of intimately mixed precipitates, nucleii of barium-sulfate crystals begin to enlarge into more or less interlocking groups, the groups enclosing some, pushing aside other zinc sulfide. This process is halted before many single crystals reach a size greater than about 0.3 μ, but groups with enmeshed zinc sulfide may have a diameter of about 1.5 μ. Heating would expel the enclosed water, and cause the zinc sulfide to crystallize. An aggregate formed of two substances by the adherence of grains less than about 0.3 μ in diameter will have an apparent refractive index determined by the proportions of the two substances. The refractive index of crystallin barium sulfate is 1.66, and of crystallin zinc sulfide is 2.37. These values are about the limits of the refractive indices of the grains of lithopone.

Zinc Oxide.—Zinc oxide (see Section X), when pure and of not too fine grain contains stout prisms with parallel extinction, positive elongation, $\omega=2.00$, $\epsilon=2.02$. The presence of even a few per cent of lead prevents to a large extent the growth of crystals, but does not hinder during reheating the process of aggregation of fine particles into dense masses 3 to 5 μ in diameter. These may have a refractive index as low as 1.94.

Harrison Red.—Harrison red consists of minute crystals, probably orthorhombic, rounded grains and stout prisms about 1 μ in diameter; elongation negative; $\alpha_{Na}=1.60$, $\beta_{Na}=$ about 2.1_{Na}, $\gamma=2.25_{Li}=2.33_{Na}$. The optical dispersion of γ is very high from the red to the green. Pleochroism pronounced, α orange, β and γ deep red. Grains are practically opaque to green and blue but transmit some violet. The violet becomes very evident if a thin film containing crystals dispersed sparsely in oil is viewed by transmitted light, especially if this color is compared with the orange red diffused by the film.[1] Heated with large excess of linseed oil the crystals dissolve to an orange-colored liquid, which deposits spicular crystals on cooling. These crystals have the same color, pleochroism and elongation as the original ones. It may be noted, however, that on account of the pleochroism with respect to shape the coloring power of the spicular crystals would not be as great as that of the nearly equi-dimensional original crystals. Also, the diffusing power of the spicular crystals would be very considerably less on account of birefringence with respect to shape. (See Section VII.)

Vermilion.—Vermilion (HgS) varies somewhat in color owing to differences in grain. Its refractive indices are very high, $\omega_{Li}=2.81$, $\epsilon_{Li}=3.14$.

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[1] The refractive index for violet must be considerably less than for red to account for this difference (compare with ultramarine).

Indian Red.—Indian red consists of very small hematite (see scarlet oxide) crystals, mostly from 0.5 to 1.0 μ, which may be aggregated into rough grains up to about 5 μ, $\omega_{Li}=3.01$, $\epsilon_{Li}=2.78$.

Scarlet Oxide.—Scarlet oxide or jewelers' rouge—hematite in smaller grains than Indian red—color brighter and possesses more orange than Indian red. Grains of hematite 3 μ in diameter would have such low transmission that a pigment consisting of them would be a dark gray with little color. Films of hematite may be thin enough to transmit a light orange-yellow color. Such plates transmit all colors, but blue and violet noticeably less than the others. Plates a little thicker are orange, and transmit little between 450 and 500 $\mu\mu$, but from 450 to 400 $\mu\mu$ transmission seems to increase.[1] It is evident that colors ranging from orange yellow to dark red or purplish can be obtained from crystallin hematite if proper means of diffusing the color are furnished. Yellowish colors could be obtained only by means which would insure complete diffusion before the light had traversed a total thickness of the hematite greater than about 0.1 μ.

Alizarin Lake (Crimson).—The isotropic base with the coloring matter has a variable, low refractive index—about 1.70 for red. Thus the diffusing power is low. The sample examined contained a small percentage of grains about 10 μ in diameter. Such grains transmit very little between wave lengths 480 and 550 $\mu\mu$, and even red is slightly absorbed. Violet is not strongly absorbed. When tinted out with white the presence of the large grains causes dullness.

Scarlet Lead Chromate.—The single sample of scarlet lead chromate examined consisted of beautiful rectangular tabular crystals 2 to 5 μ thick and from 3 to 10 μ in other dimensions. The extinction is parallel, γ vibrates normal to the plates and α parallel to the elongation. α is orange-yellow γ and β are orange-red. $\alpha_{Na}=2.42\pm0.01$, $\beta=2.57_{Li}$ or 2.7_{Na}, $\gamma_{Li}>2.57<2.62$. The symmetry is probably orthorhombic.

This even-grain, coarse-grain sample when mixed with oil had a pure, slightly darkened color—between *5 h* and *5 i* of Ridgway's scale. It maintained its hue and purity when darkened with ivory black. When brightened with white it maintained its hue but produced slightly dull tints—*5′ d* and *5′ f*.

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[1] These observations were made on flakes of hematite in aventurine feldspar furnished by Mr. Olaf Andersen of the Geophysical Laboratory. He calculates the thickness of yellow plates as 0.1 μ, *American Journal of Science*, Vol. 40, p. 370 (1915).

Some of the dry pigment was ground in oil until its hue changed to near orange—*9 h*. It was now very uneven grained, many grains of the original size remained and a small percentage of the volume was as fine as 0.5 μ. It still tinted out dull on account of the presence of the large grains, and the small grains caused the hue to migrate a good deal when it was darkened with black—to 13 μ.

Red Lead.—Red lead is known in three different forms, as follows:

1. The mineral "minium" occurs in needle-like crystals with parallel extinction, which are strongly pleochroic from yellow to red.

2. Scaly or irregular grains which are crystal units without definite outlines, are formed commercially by oxidizing litharge flakes. As oxidation proceeds from points on the surface of the flakes the crystals of red lead replace the litharge. Imperfect oxidation can be detected by observing the optical properties of the product (see litharge). These grains of red lead are strongly pleochroic, show anomalous green interference colors, have very slight birefringence and a refractive index of 2.42_{Li} and 2.5_{Na}. Some samples which are relatively coarse grained, 20 to 50 μ diameter, are almost completely oxidized, while others which contain relatively small amounts of grains larger than 20 μ, contain more than 10 per cent of unoxidized material.[1]

3. Red lead, which shows almost no crystallin structure and appears as friable aggregates of particles which are exceedingly small, is prepared from lead monoxide derived from lead carbonates.

The color of these several products varies. The third is the lightest. In the first and second, size and shape of grain are factors determining color—coarse-grain material is darker or redder except when poorly oxidized.

Burnt Sienna.—Burnt sienna is very uneven grained, but contains much very fine material. It shows no evidence of crystallinity, grains have variable, moderate refractive index,—around 1.85,—and are fairly transparent to red, orange and yellow. One effect of its diversity of grain is worthy of notice. White, added gradually, first intensifies the color, then because of the large grains the shades become dull as the white acts as a diluent.

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[1] A series of analyzed samples was furnished for these studies by P. H. Walker of the U. S. Bureau of Standards.

Massicot in its commercial form is a yellowish, powdery form of PbO. Natural yellow crystals of PbO have been known for many years as massicot. The crystals are orthorhombic scales or thin plates, faintly pleochroic with γ most strongly absorbed, $\alpha_{Li}=2.51$, $\beta_{Li}=2.61$, $\gamma_{Li}=2.71$. PbO which has crystallized from fusion in dull orange flakes is known commercially as *litharge.* Microscopical examination shows the flakes to consist largely of yellow massicot. The orange color is due to reddish streaks, of another, tetragonal, crystallin form of PbO for which no definite name exists, although it has been called red litharge. It is not noticeably pleochroic, $\omega_{Li}=2.66$, $\epsilon_{Li}=2.53$.[1]

Chrome Orange.—Not enough samples of this pigment have been studied to yield definite results. The grains are not well defined crystals as in the case of the three other lead chromates described, and the pigment is not essentially a mixture of such crystals. The refractive indices are intermediate between those of the scarlet chromate and chrome yellow medium.

Barium Chromate.—No distinct crystals were seen in the one sample examined. Particles 2 to 5 μ in diameter consist of globules or lumps with irregular double refraction, $n=1.94-1.98$.

Cadmium Yellow.—See *American Journal Science*, Vol. 34, pp. 365–387.

Zinc Yellow.—One sample examined appeared like barium chromate, but n is lower, $n=1.84-1.90$.

Lemon-Chrome Yellow.—Only one out of six samples of this pigment from various sources was not distinctly crystallin. This one consisted of formless lumps and fine grains having a refractive index of 2.0 to 2.1 for yellow.[2] It was among the yellowest—22. The other samples were composed largely of prismatic or rounded grains 1 μ or less in diameter, extinction parallel, elongation γ; refractive indices Na : $\alpha = 2.10$–2.12, $\beta = 2.2(1)$, $\gamma = 2.38$–2.43. The crystals are probably orthorhombic, and are optically related to lead sulfate, though differently elongated. The problem of their exact relation to lead sulfate requires a study of conditions of precipitation in relation to optical (and possibly other physical) properties and composition.

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[1] The relation of these forms in nature has been studied by E. S. Larsen, Jr., *Am. Min.*, Vol. 2, p. 18 (1917). These refractive indices were partly measured on crystals made by L. H. Adams of the Geophysical Laboratory.

[2] Analysis furnished by P. H. Walker showed 26.98 per cent lead sulfate, and for one of the crystallin samples 35.02 per cent. This was not present in separate crystals.

Chrome Yellow Medium.—Samples from four sources all consisted essentially of prismatic crystals mostly less than 0.7 μ in diameter and 2 to 6 μ long. The crystals extinguish parallel, have positive elongation, and refractive indices γ 650 $\mu\mu = 2.49$, α 620 $\mu\mu < 2.31$. Analysis furnished by P. H. Walker showed 5 to 11 per cent excess PbO over $PbCrO_4$.

Chromic Oxide.—Several samples were examined. All consisted of porous lumps in which individual particles could not be distinguished. The most compact material showed considerable transparency, even for red and orange, and noticeable absorption in the green. These characteristics account for the dullness of its color, especially in mixtures. Its refractive index is high, but could not be found accurately. The value $n_{Li} = 2.5$ was obtained. Probably this is about the value for green and violet also, for in the hydrated oxide dispersion is very low.

Chromic Oxide ("transparent").—This is probably a hydrated oxide without crystallinity. Lumps 20 μ in diameter are dull greenish with absorbing properties similar to the anhydrous oxide but much weaker. $n = 1.625$–1.645 for yellow and is practically the same for other colors.

Verdigris.—Irregular crystal fragments, pleochroic from lighter blue-green, γ, to deeper green-blue, α; $\alpha = 1.53$, $\gamma = 1.56$; 2 V very large, and the bisectrices strongly dispersed. Grains 30 μ in diameter transmit considerably throughout the spectrum.

Cobalt Blue (cobalt aluminate).—Grains vary in depth of color; those most deeply colored have the highest refractive index, which for blue is 1.74, for red a little > 1.78, and for green < 1.73. Grains 10 μ in diameter are practically opaque to wave-lengths between 560 and 610 $\mu\mu$. Wave-lengths in the red longer than 650$\mu\mu$, and blue and violet, are freely transmitted. Considerable colorless isotropic material of refractive index about 1.70 was found adherent to and mixed with the colored grains. The refractive index of the colored grains is about what would be expected of crystallin $CoO.Al_2O_3$, that is, cobalt spinel.[1]

Ultramarine.—The grain of ultramarine is about that of Dutch white lead. Particles of medium size are pale blue and isotropic. Their refractive indices cannot be measured for sodium light because they are opaque to it. They are fairly transparent for red light, for which the refractive index is about 1.63 for Li-light. Measured in green light wave-length 520 $\mu\mu$ the refractive index is 1.51; in the violet light at 450 $\mu\mu$ it is 1.535. Thus the dispersion is anomalous, because the refractive indices are lower for green and violet than for red.[2]

Permanent Violet, a manganese compound.—Crystals probably orthorhombic 4–8 μ in diameter, in flat double pyramids with corners truncated. β, perpendicular to plane of flattening, $=$about 1.72 for violet; α, parallel to greatest elongation, $=1.64$ orange, 1.65 green, 1.67 violet; $\gamma=1.79$ orange, 1.73 green, 1.75 violet. 2 V is about 45 deg. γ and β are violet-red, α is colorless or light gray. If α were perpendicular to the plane of flattening the pigment would have more color, perhaps 25 per cent, and more diffusing power. The sample examined contained some ultramarine.

The Lakes.—The colorless bases which receive the coloring matter have refractive indices which are low and variable, 1.58–1.72 usually. The coloring matter slightly affects the refractive index. The particles of the lake vary from very coarse to very fine. Lakes with coarse to very fine particles, and refractive index about 1.60, are very transparent in thin films with oil, and are thus not so good as glazing colors. Fine-grain lakes having refractive indices 1.65–1 72 are only semi-transparent in such films and thus are not so good as glazing colors. The film, if made between glass slips, may diffuse a noticeably different color from what it transmits.

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[1] See J. A Hedvall, Arkiv, Kem. Min. Geol., Vol. 5, No. 16 (1914).

[2] Small amounts of lighter colored materials are intimately associated with the ultramarine. These do not vary in refraction like the ultramarine, but they have when in a refractive liquid practically the same apparent refraction as ultramarine in ordinary light. Thus, although the heterogeneity of the material is not apparent in ordinary light, it manifests itself at once in green or red light and makes refraction determinations difficult.

FIG. 1.

Each apex in Fig. 1 represents 100 per cent of either white, black or full color. The percentage of each of these decreases regularly to 0 at the opposite side. Tints containing no black—along *CW*—may be called clear tints, and shades containing no white—along *CB*—may be called pure shades. These terms may be applied in practice to tints and shades containing respectively a little black or white, which would be overlooked in a casual examination. Dull (broken) colors occur over the interior of the triangle, neutral grays along *WB*, and colored grays adjacent to *WB*.

FIG. 2.

Fig. 2 shows the positions of the Ridgway colors. The subscript *R* is used to indicate that the colors are made up, not of theoretically perfect black, white, and full color, but of standards specified by Ridgway. When color analyses of these standards are available it will be found that for each of the standard hues (full colors) and their modifications by black, white and gray the lettered positions on a triangle representing the analyses will be different. The symbols for intersections not marked can be inferred.

FIGS. 3 TO 7.

The curves in these figures are selected from a larger number of curves to illustrate typical conditions. Different samples of the same pigment give the same type of curve, with rare exceptions, but not necessarily the same curve. Some substances which are not ordinarily used as pigments have been studied because of their illustrative value.

FIG. 3.

White pigment added to colored paints. (See Section XIV.) Increasing white causes changes shown by arrows. Some changes follow the boundaries *B* to *C* to *W* (true cobalt blue), or *C* to *W* (cadmium orange, zinc yellow). A very fine-grain white produces with Indian red or lamp-black the curves "a", but a white containing no very fine grains produces the curves "b".

FIG. 4.

Black pigment added to colored paints. (See Section XIII.) A non-diffusing black pigment like ivory black when added to a paint or moistened powder consisting of a single kind of grains and having a certain proportion of white in its diffused color should diminish the white and color almost proportionately. That is, the curve of darkening should be a nearly straight line across an ideal triangle, joining the position of the color with a very dark gray (containing about 4 per cent white). On the triangle here shown the true proportions of white and color are not exactly represented, therefore the curves for the single pigments may not be of true form. The tints produced by mixing color with a strongly diffusing white must, when gradually modified by admixed black, lose color much faster than they lose white, because white grains assist largely in diffusing the color transmitted by colored grains. The three such curves on the diagram are concave in the proper direction, but the one representing white plus yellow lake cannot be an ideal curve, for white appears to increase during the first additions of black. The fault is not in the color comparisons but in the standards. The curvature for a strongly diffusing colored pigment is less than for one which is weakly diffusing. (Compare vermilion and alizarine lake.)

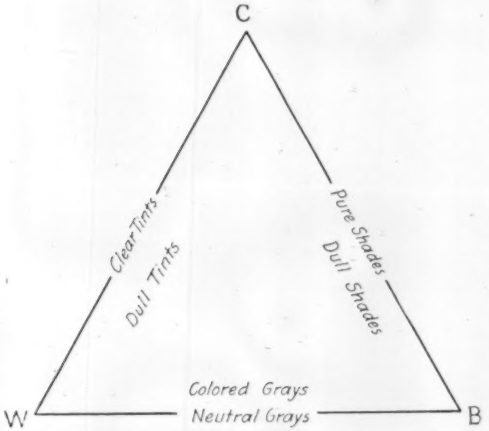
C
Clear Tints
Dull Tints
Pure Shades
Dull Shades
Colored Grays
Neutral Grays
W
B

FIG. 1.

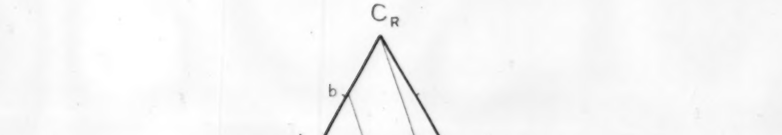
C_R
b
d
b'
d'
f
i
f'
b''
d''
i'
k
f''
b'''
d'''
i''
f'''
i'''
m
d''''
b''''
f''''
d'''''
b'''''
W_R
B_R

FIG. 2.

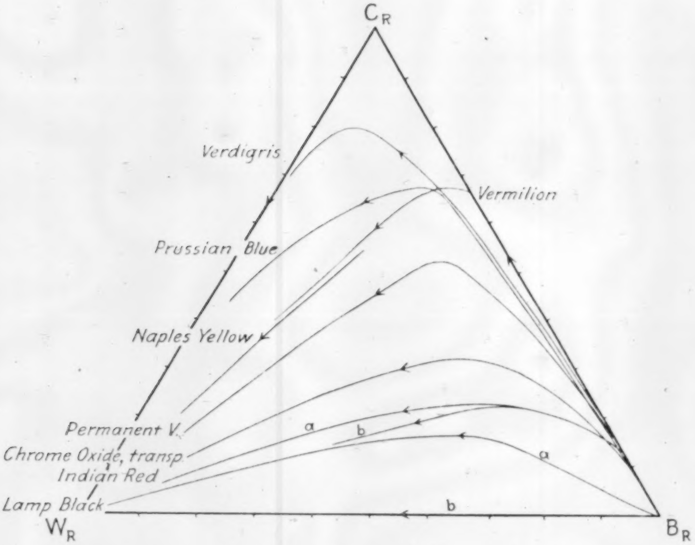

FIG. 3. (see p. 522).

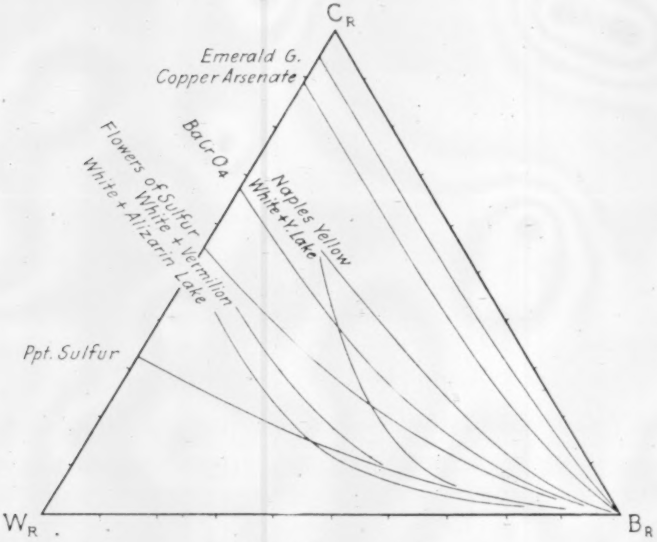

FIG. 4. (see p. 522).

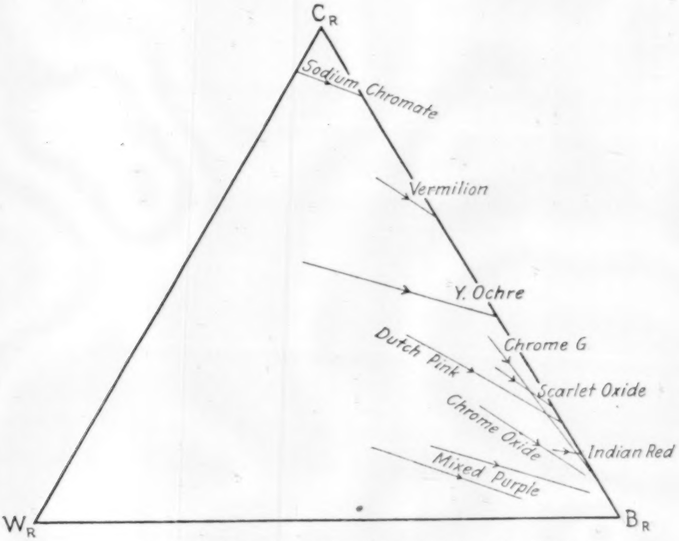
C_R
Sodium Chromate
Vermilion
Y. Ochre
Chrome G
Dutch Pink
Scarlet Oxide
Chrome Oxide
Indian Red
Mixed Purple
W_R
B_R

FIG. 5.—Wetting (mixing with oil) causes the color of these pigments to become more nearly pure but less nearly full.

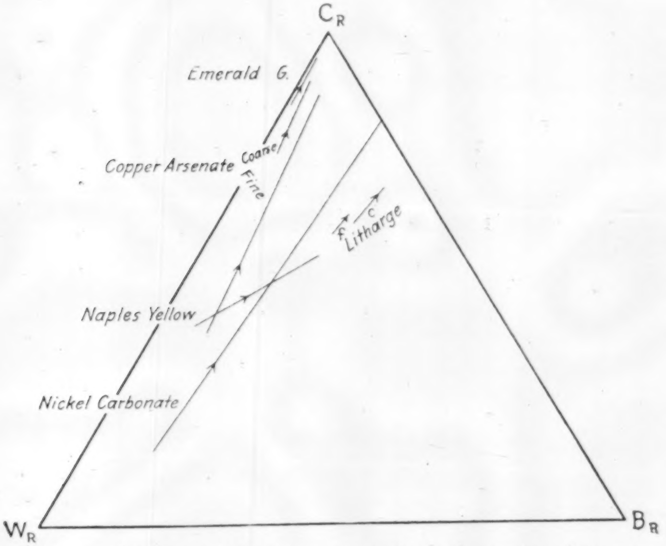
C_R
Emerald G.
Copper Arsenate
Coarse
Fine
f
c
Litharge
Naples Yellow
Nickel Carbonate
W_R
B_R

FIG. 6.—Wetting causes the color of these pigments to become more nearly pure and full. (See Section XV.)

FIG. 7.

Fig. 7 is designed to show changes in hue accompanying the addition of white or black pigments to colored paints (Sections XIII and XIV); but changes in purity and brightness are also indicated roughly. At the top the odd numbers 1 to 71 represent the Ridgway hues. Thus, intermediate hues between red (1) and orange (11) are 3, 5, 7, 9, and between violet (59) and red are 61, 63, 65 (purple), 67, 69, 71. In the upper part of the figure the colors are full, or nearly so, along the central horizontal line and grade upward (*b*, *d*, *f*) into clear tints, and into pure shades downward (*i*, *k*, *m*); in the lower part the colors are dull. Toward the top they pass into dull tints and toward the bottom into dull shades. (See Section V, last part.) *b* represents 9.5 per cent white; *d*, 22.5; *f*, 45; and *i* represents 45 per cent black; *k*, 70.5; *m*, 87.5. (See Fig. 2.) Circles, and sharp breaks in curves, represent the various colors before they have been modified by adding black or white. The curves marked "a" were produced by using a very fine-grain white, those marked "b", or not marked at all, by using a white containing few very fine grains.

The indices (′) to (′′′′) indicate that 32, 58, 77, and 90 per cent of Ridgway's neutral gray is present in the color besides the black or white represented by the letters.

Harrison red is a slightly dark orange-red (about 5 *i*). Added white scarcely affects the hue unless the white contains much very fine-grain material, in which case the hue changes to red (1 f^1), and the tints produced are slightly dull. Black added causes the hue to become slightly more orange.

Yellow lake alone is a very dark orange-red (near 9 *m*). Added white causes the hue to migrate through orange to yellow. The color increases to a slightly dull orange-yellow, which then gives place to tints which become more yellowish as they grow lighter.

Lemon yellow (chrome) is a nearly pure, slightly orange yellow. White changes its hue to yellow, and black, to greenish yellow (27 *m*).

Note the difference between an even-grain cadmium orange and a darker, poorly ground, uneven-grain one. Also between a coarse-grain darker potassium chromate and one that is fine grained.

MERWIN ON COLOR OF PIGMENTS.

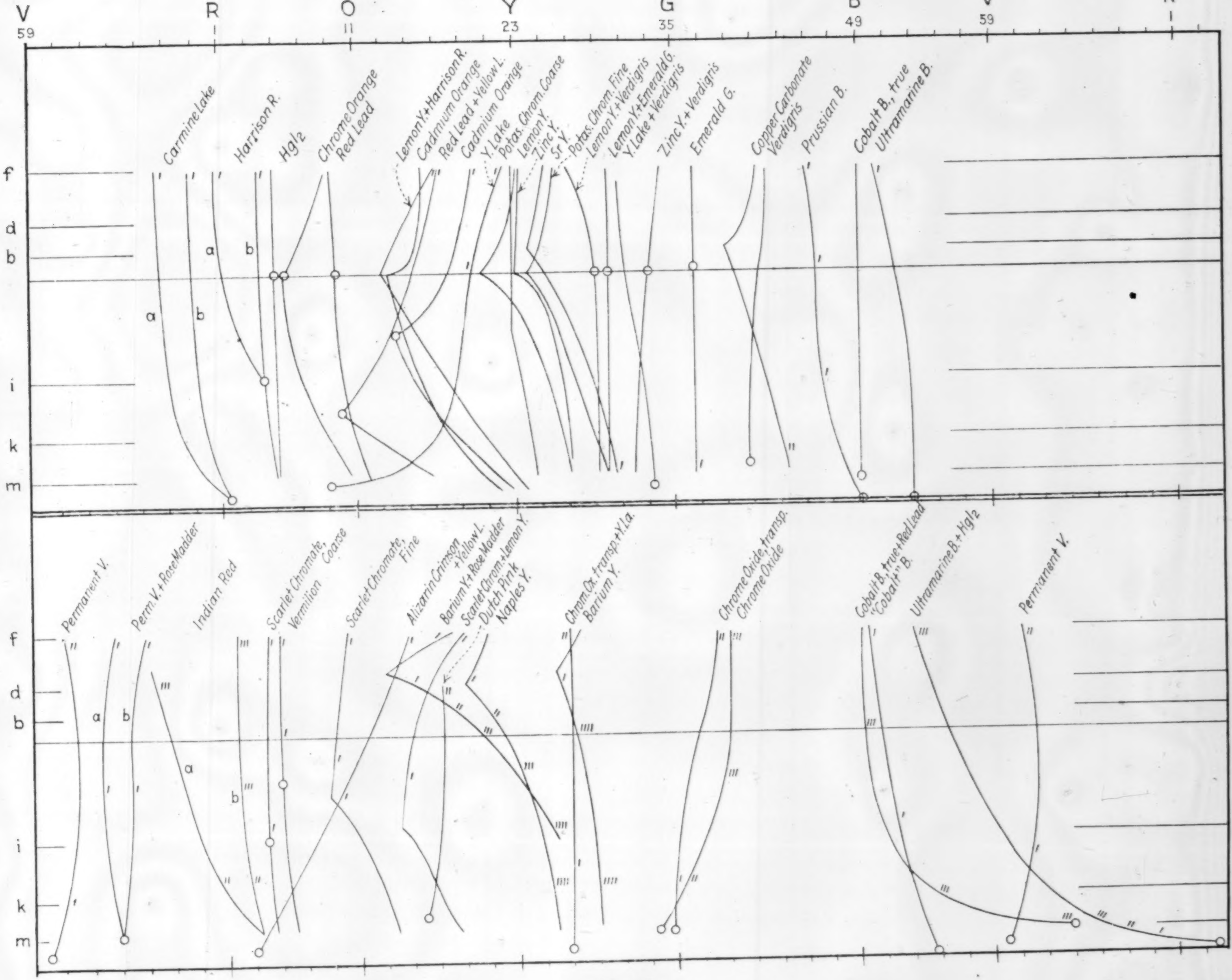

FIG. 7.

DISCUSSION.

Mr. Walker.

MR. P. H. WALKER.—I am not prepared really to discuss this paper. Mr. Merwin is speaking largely in a language with which I am not familiar. It is, in fact, a language with which I think most of the members of Committee D-1 on Preservative Coatings are not familiar, and we shall have to study this paper very carefully in order to get the meat of it. I want particularly to urge the members of Committee D-1, and members of the Society who are not familiar with the use of the microscope, to read this paper—not just once and say "This is beyond me," but several times, for it will be found that the paper contains many points bearing directly on the subjects that we are most vitally interested in.

Now, having stated that I really am not competent to discuss it, I want to ask a question in regard to the simplest portions of this paper. Mr. Merwin has determined, to a degree that has never been approached before, the fineness of a large number of pigments. The dimensions are expressed in microns, and he has necessarily examined an exceedingly small proportion of the material in hand. We all know how difficult it is to get a representative sample, and I should like to ask him to give an expression as to the degree of confidence he has in making, in a reasonable length of time, sufficient microscopic measurements of a given lot of pigments to be able to say with a fair degree of certainty that the samples which he has examined are representative of the mass of material to be considered.

Mr. Merwin.

MR. H. E. MERWIN.—Few dry pigments can be satisfactorily sampled with respect to size of grain. A rapid examination of several portions of a sample of paste will usually indicate the procedure necessary to secure results which are representative.

The Chairman.

THE CHAIRMAN (VICE-PRESIDENT S. S. VOORHEES).—I am confident that much of the discussion which we had this morning regarding the different types of red leads and their different behaviors[1] will, in a measure, be answered by the methods which

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[1] See pp. 539 ff.—ED.

Mr. Walker. Mr. Merwin uses in his investigations, and I believe that the members not only of Committee D-1 but also of the Society at large will make use of these methods for determining these factors.

Mr. Lasier. MR. E. L. LASIER (*by letter*).—Indirectly related to the matter considered by the author's paper is the question of the duplicating or matching of colors. The writer would be glad to have Mr. Merwin comment upon that subject.

Pigments differ in shade, tone, fineness, tinting strength, or in chemical composition, and to require manufacturers to furnish pigments identical in all physical characteristics with, say, a standard sample made by one firm, would arbitrarily compel each manufacturer to duplicate his competitor's products. Thus in painting operations where only a limited supply of paint is mixed at one time, the problem presents itself of so modifying the mixtures as to produce the same or approximately the same shade and color. The writer says "approximately the same" because, in all practical work, it is impossible to reproduce a color to an absolute exactness.

Since paints are usually made according to certain specified formulas, it is invariably necessary to correct a paint during manufacture so as to produce the desired color, because of the variations in shade and tinting strength, due to the fact that the products of no two manufacturers are identical. It is at this point that color strips or panels are of value.

The use of color panels, however, presents difficulties. Accumulative changes occur in the color of such panels. The personal equation in matching colors through the medium of panels is present. Possibly the variation in color of panels prepared at different times from the same formula is due to a question of manufacture. The variation may be due to the little known effects of the environment under which the panels are kept—as for instance some colors require darkness to enable them to retain their shade, while others, such as white paints, need the bleaching action of light. Again, some paints matching a panel freshly painted from a standard paint sample, markedly differ in color when they dry out.

The Navy Yards in 1906 were furnished standard samples of various dry and oil pigments from one yard; replenishment of those standard samples was contemplated with samples taken from commercial deliveries. This establishment of standard pigment samples for most pigments used in Navy work has been the basis of obtaining duplicate colors in painting work conducted at the different yards. The method cannot, however, be called altogether successful, since panels prepared at one yard at different times from different standard samples of supposedly the same pigment, have varied appreciably in shade and color. **Mr. Lasier.**

The suggestion has been offered, relative to the question of the standardization of colors, of establishing standard panels, standard pigment samples and standard paint samples. The suggestion is also being considered of utilizing some colorimeter, by means of which a permanent record can be obtained of any color (practically independent of the personal equation, environment, etc.), which can be used as an index or key for that color in duplications. If such an instrument could be developed into a form sufficiently practicable to permit its use without requiring that the manipulator be a trained scientist, it would undoubtedly meet with approval by industrial interests using paints and pigments to a large extent. Possibly, too, glass cubes, one or two inches on an edge, filled with the paint and then hermetically sealed, might be used in a way similar to those employed by the Bureau of Standards as standards for grading cotton-seed oils and rosins.

The writer believes the subject worthy of consideration by investigators in the membership of this Society.

Mr. Mervin. MR. MERWIN (*Author's closure by letter*).—With rare exceptions paints made according to approximately the same formula will match satisfactorily after drying if they match when freshly made. Could not, then, the standard color in form of paste be put up in small collapsible tubes containing enough to paint a sample panel to be used freshly painted to compare with the paint being mixed?

Or a layer of thick paste between glass plates would remain indefinitely without drying, and could be used to compare with other pastes between (or under) glass plates. Comparisons can be made less satisfactorily under glass, for the thickness and quality of glass as well as the thickness of the layer of paste would need consideration; also, glass eliminates certain surface

Mr. Merwin. characteristics of the paint film which under oblique illumination are not negligible.

In practical work direct comparisons are preferable to colorimetric analyses. The latter are valuable in research, in reproducing a lost standard, or in determinations in which direct comparisons are not feasible. But from them a lost standard cannot, in general, be reproduced as accurately as it could have been reproduced by direct comparison.

METAL PRIMER TESTS.

By H. A. Gardner.

SUMMARY.

A series of exposure tests was made on paints applied to large-size metal plates, 24 by 36 in., with the object of determining what grade of pigment is best suited for use as a priming coat.

After three years' exposure, the panels to which had been applied a single coat of red lead containing a substantial percentage of litharge were in better condition than the panels painted with a single coat of highly oxidized red leads or iron-oxide paints. Those portions of the panels to which had been applied two coats of paint, gave substantially the same relative results, much greater durability being shown as compared to the single-coated surfaces.

The paints used were reserved, examined and analyzed after storage for three years. The highly oxidized red leads in general were not in superior condition to the others, except in one instance where a red lead of low specific gravity was used. The other paints were soft and in excellent condition.

The formation of lead glycerinate and its value as a preserving material is discussed.

METAL PRIMER TESTS.

By H. A. Gardner.

A series of test panels to determine the efficiency of basic pigments as primers for metal was exposed at the Institute of Industrial Research during April, 1914. One object of the tests was to ascertain whether red lead of high or low basicity is best suited for protecting metal from corrosion. Other pigments which are known to be good metal protectives were included in the tests, in order to have a definite standard upon which to base the service of the red leads.

Paints and Application.

The paints were all applied to the same grade of black iron plates, 24 by 36 in., free from rust. The first coat of each paint was applied to the entire surface of the plate at a spreading rate of 600 sq. ft. per gallon. The plate was divided by a diagonal line from the upper right-hand to the lower left-hand corner, and the second coat was applied only to the lower right-hand half of the plate, at a spreading rate of 900 sq. ft. per gallon. Four days were allowed for drying between coats. The paints were all applied to the panels after they had been attached to the supporting fence frame. The red-lead paints were prepared so that they would all be practically of the same viscosity or body. A standard formula of 25 lb. of dry pigment to 1 gallon of oil was found practical except with the pigments on panel No. 6, which required more oil to bring it to working consistency and to relatively the same viscosity as the others.

The actual compositions, as determined by analysis, of the paints after preparation are shown in Table I.

Results of Tests.

In previous tests[1] the writer has observed that red lead which has been highly oxidized during production, and which

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. IX, pp. 208 ff. (1909); Vol. X, pp. 409-410 (1910); Vol. XI, pp. 192, 641 (1911); and Vol. XIII, pp. 369, 954 1913).

TABLE I.—COMPOSITIONS OF PAINTS USED.

Panel No.	Pigment.	Grade: Lead Tetroxide Content, per cent.	Pigment in Paint, per cent.	Linseed Oil in Paint, per cent.	Weight per Gallon, lb.
1	Red Lead.............	86[a]	80	20	27
2	" "	88[a]	80	20	25
3	" "	90[a]	79	21	28
4	" "	93[a]	78	22	24
5	" "	95[a]	80	20	27
6	" "	99[a]	72	28	21
7	" "	98[b]	80	20	24
8	Sublimed Blue Lead....	Contains 1 per cent added Carbon Black	63	37	16.5
9	Chromated Red Lead...	Contains 2 per cent CrO_3..............	76	24	22
10	Chromated Iron Oxide..	Contains 1 per cent CrO_3..............	56	44	14
11	Iron Oxide............	Contains 10 per cent Zinc Chromate ...	55	45	14.5
12	" "	Contains 20 per cent Zinc Oxide.......	58	42	15

[a] Balance is litharge.
[b] Special red lead purchased in paste form.

TABLE II.—RATING OF PAINTS AT INSPECTION APRIL 1, 1917, AFTER 3 YEARS' EXPOSURE.

10=EXCELLENT; 1=FAILED.

Panel No.	Pigment.	Grade: Lead Tetroxide Content, per cent.	One-Coat Work.	Two-Coat Work.
1	Red Lead.............	86[a]	7	10
2	" "	88[a]	8	10
3	" "	90[a]	8	9
4	" "	93[a]	7	8
5	" "	95[a]	6	7
6	" "	99[a]	2	5
7	" "	98[b]	4	6
8[c]	Sublimed Blue Lead ...	....	....	....
9	Chromated Red Lead...	Contains 2 per cent CrO_3..............	8	10
10	Chromated Iron Oxide..	Contains 1 per cent CrO_3..............	2	10
11	Iron Oxide............	Contains 10 per cent Zinc Chromate....	2	9
12	" "	Contains 20 per cent Zinc Oxide.......	1	9

[a] Balance is litharge.
[b] Special red lead purchased in paste form.
[c] Panel injured when exposed. Removed from test.

is therefore practically a neutral pigment, is not as well suited for application to metal as red lead which is highly basic in nature and which contains a considerable percentage of litharge, the latter being a highly basic pigment. In the present tests the results obtained have been similar and have demonstrated the superior value of the incompletely oxidized or highly basic red leads. See Figs. 1 and 2. This was shown by an inspection of the paints at the end of 3 years' exposure, the results of which are recorded in Table II.

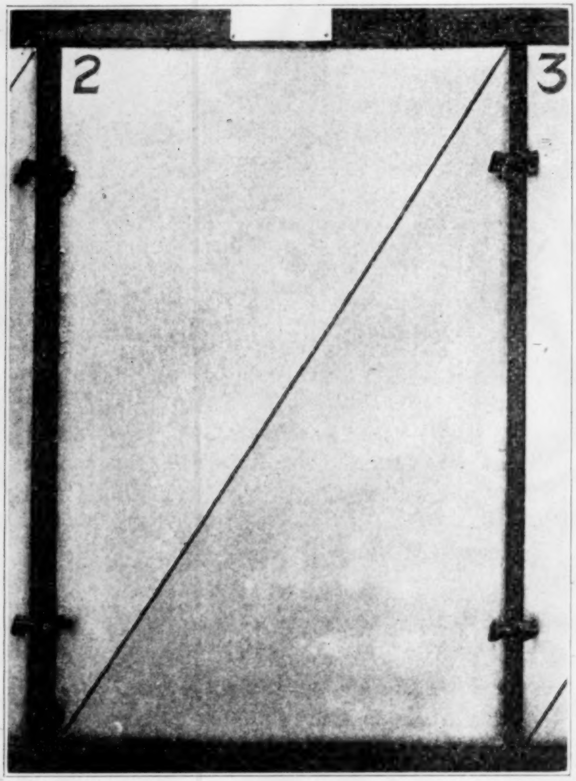

FIG. 1.—Panel No. 2; 88 per cent Red Lead. Photographed after 3 Years' Exposure.

As a further result of the tests, it is quite apparent that two coats of the neutral iron-oxide paint (chromated or containing zinc chromate or zinc oxide) are superior to two coats of the neutral red-lead paint. The tests also demonstrate the excellent results to be obtained with one coat of a highly basic red lead. One of the most marked results from the tests is shown in Fig. 3, which illustrates the comparative durability of one and two-coat work on an iron oxide - zinc oxide paint. This result would indicate that all metal should preferably be given at least two coats of paint when erected.

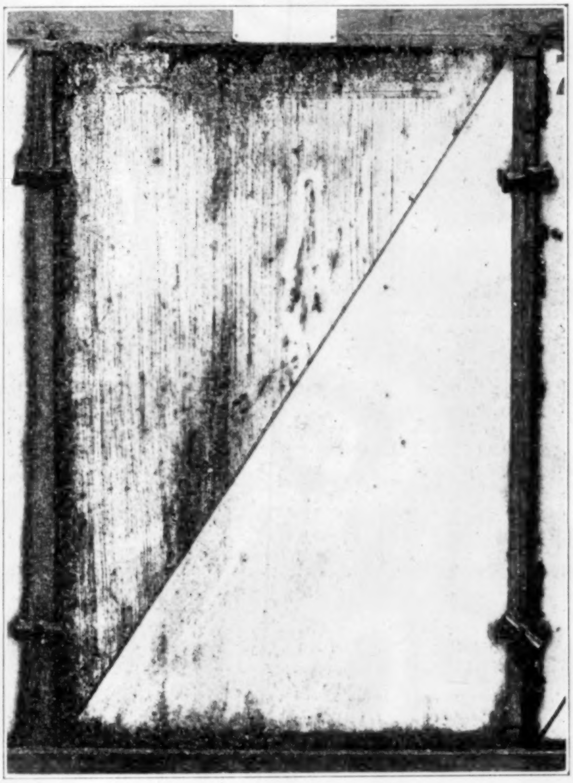

FIG. 2.—Panel No. 6; 99 per cent Red Lead. Photographed after 3 Years' Exposure.

STORAGE PROBLEMS.

Much has been said regarding the hardening of red lead in packages, especially if the red lead should contain any pronounced percentage of litharge. It is interesting to record

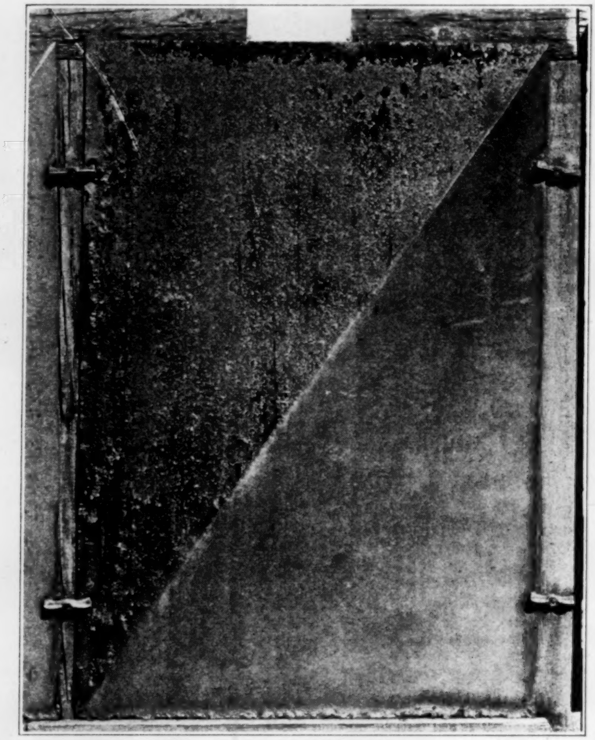

FIG. 3.—Panel No. 12; 80 per cent Iron Oxide, 20 per cent Zinc Oxide. Photographed after 3 Years' Exposure.

the condition of the paints used in these tests after standing for three years in half-gallon cans. When the cans were opened, some of the clear oil was removed from each package and ashed to determine the amount of pigment that had gone into solution. The results of the tests indicate that red leads high in lead tetroxide are quite as soluble in linseed oil as red leads high in lead monoxide. The condition of the paints observed at the time of opening is also recorded in Table III, which would seem to indicate that the softness of red-lead paints after aging is due not entirely to the amount of litharge contained therein but also to the settling properties of the red lead. In other words, red leads of high weight per volume will settle down to the bottom of the cans and become hard, whereas red leads of low specific gravity are more likely to be maintained in a bulky form.

TABLE III.—EXAMINATION OF PAINTS USED IN TESTS AFTER STORAGE FOR 3 YEARS.

Panel No.	Pigment.	Grade: Lead Tetroxide Content, per cent.	Condition.	Pigment Dissolved in Oil as Shown by Ash, per cent.
1	Red Lead	86[a]	Stiff	2.55
2	" "	88[a]	Stiff	1.22
3	" "	90[a]	Fairly stiff	5.24
4	" "	93[a]	Stiff	15.82
5	" "	95[a]	Stiff	1.49
6	" "	99[a]	Fairly soft	0.31
7	" "	98[b]	Stiff	3.90
8	Sublimed Blue Lead	Contains 1 per cent added Carbon Black	Very soft	1.61
9	Chromated Red Lead	Contains 2 per cent CrO_3	Soft	0.80
10	Chromated Iron Oxide	Contains 1 per cent CrO_3	Very soft	0.50
11	Iron Oxide	Contains 10 per cent Zinc Chromate	Very soft	1.48
12	" "	Contains 20 per cent Zinc Oxide	Very soft	1.22

[a] Balance is litharge.
[b] Special red lead purchased in paste form.

The results obtained in the storage tests indicate that some chemical action takes place other than that to be accounted for by the formation of lead linoleate, since the latter is soft rather than hard. The writer is of the opinion that the chemical reactions which are partly responsible for the hardening of red-lead paints, cause the formation of lead glycerinate, a substance that is recognized as one of the hardest and most durable cementing materials. It is obvious, therefore, that linseed oil having a high acid number and often containing a substantial amount of free glycerin, is dangerous to use in making red-lead paints that are to remain in packages for a long period previous to use.

With linseed oil it is probable that a substance having a solvent effect upon lead glycerinate, such, for instance, as certain alcoholic derivatives, could be combined. These would evaporate when the paint was applied and the formation of the valuable lead glycerinate cement might proceed during the drying of the oil. For this purpose, red leads high in litharge would be most active.

DISCUSSION.

Mr. Sabin.

MR. A. H. SABIN (*presented in written form and read by the author*).—An examination of Table I seems to show some mistakes in the recorded composition of the red-lead paints. In the first place, it is stated that a standard formula of 25 lb. of dry pigment to 1 gallon of oil was adopted for panels Nos. 1 to 5, and 7; the percentages given in the tables are 78, 79 and 80 for the pigments and 22, 21 and 20 for the oil. It is obvious that 25 lb. of pigment and 7.75 lb. (=1 gallon) of oil would represent 76.33 per cent of pigment and 23.67 per cent of oil; on the other hand if the proportion aimed at was 80 to 20, that would be equivalent to 31 lb. of pigment to a gallon of oil.

Examining the table further, it appears that paint No. 1 weighed 27 lb. per gallon; the red lead contained 86 per cent of Pb_3O_4. Assuming the density of this red lead to be about 8.7, it would take about 73 lb. of it to make the volume of a gallon in a paint; that is, with the spaces between the particles filled with oil, which latter is reckoned separately. A gallon of such paint made with 25 lb. of pigment to 1 gallon of oil would weigh 24.3 lb. instead of 27 lb. as stated in the table. On the basis of 27 lb. per gallon, the proportion of pigment to oil is 21.5 lb. to 5.5 lb. or 79.6 per cent to 20.4 per cent, which is pretty near the figures in the table.

Paint No. 2 weighed 25 lb. per gallon; this would apparently require 77.6 per cent of pigment to 22.4 per cent of oil, or about one-ninth more oil than the table states, and less pigment. Paint No. 3 weighed 28 lb. and must apparently have contained 81.3 per cent of pigment instead of 79 per cent as stated, and 18.7 per cent of oil instead of 21 per cent as stated; an error of about the same amount as in No. 2, but in the other direction. Paint No. 4 weighed 24 lb.; this must apparently have contained 75.8 per cent of pigment and 24.2 per cent of oil, instead of 78 and 22 per cent as stated. If my deductions are correct, it really contained 0.5 lb. less lead and 0.5 lb. more oil than the printed figures show—a considerable difference.

Mr. Sabin. Paint No. 5 was like No. 1; the figures are not far from being consistent; but not 25 lb. to a gallon. Paint No. 6 weighed 21 lb. and seemingly contained 69.5 per cent of pigment instead of 72, and 30.5 per cent of oil instead of 28. These again are material errors, the largest yet noted. Paint No. 7 weighed 24 lb. and must apparently have been made with 75.8 per cent of pigment instead of 80 as stated, and 24.2 per cent of oil instead of 20, the latter being an error of more than a fifth of the oil, the largest in the whole set.

When Mr. Gardner says these paints got hard on standing three years, it is not to be doubted; but whether this was due to the way they were made or to their composition, is a question to which a reliable answer is no longer possible. Likewise it was to be expected that they would show, in exposure tests, great variations from the behavior of paints more skilfully made, and they did.

The determination of specific gravity of pigment is not quite as absolute as in the case of large crystals, but it is accurate within certain narrow limits. The specific gravity of litharge is 9.3; that of true red lead is probably 8.5. Pure Pb_3O_4 is difficult to make and its absolute specific gravity is correspondingly uncertain; but it is not over 8.5, and while some samples have shown an apparent density as low as 8.4, it is not impossible that these contained a little air or other gas which obstinately adhered to the fine particles. Mixtures of true red lead and litharge are capable of accurate analysis, and the densities of these commercial red leads agree very closely to the supposition that Pb_3O_4 has specific gravity of 8.5. Hence the calculation of the composition of a red-lead paint from the weight per gallon is a matter of arithmetic.

Many years ago there was some interest in apparatus for determining the weight of a unit volume of loose, dry pigment; it was hoped that an approximate notion of its fineness might thus be had. But experience showed that such apparatus would give results varying 10 to 20 per cent in the hands of the same operator; and, more important, that in many cases, and notably in that of red lead, this loose volume weight could be increased, sometimes 50 per cent, by prolonged grinding in a pebble mill. A year or two ago the writer sent a circular letter

Mr. Sabin.

of inquiry to the testing laboratories of all the railroads and many of the large manufacturers in this country, and without exception they had abandoned or had never used this method for estimating fineness. Among the railroads were the Great Northern, the Northern Pacific, C. M. & St. P., C. & N. W., U. P., S. P., A. T. & S. F., L. & N., N. Y. C., Pennsylvania, P. & R., Southern, B. & O., B. & M., N. Y. N. H. & H., B. & A., and the Erie; also the General Electric, Westinghouse, Sherwin-Williams, Lowe Bros., Masury, National Lead and the various U. S. Government laboratories. It appears, therefore, that the only means for computing paint volumes and weights are actual specific gravity determinations, which are within the reach of every one; and there is seemingly no good excuse for such mistakes as this paper exhibits.

Mr. Gardner thinks his experiments show that the softness of red-lead paints after aging is probably due to the settling of pigment of high specific gravity, while those of lower density remain soft. Thus he found that red lead of 99 per cent of Pb_3O_4 remained soft, while 98 per cent became hard. But the difference in specific gravity between these is less than 0.01, and it is doubtful if so small a variation can account for this behavior, especially as it is well known that the U. S. Government has no difficulty in buying hundreds of tons of a mixture of 98 per cent red lead with raw linseed oil under a guarantee that it will not become hard. The difference is probably due to some unknown variation in making or storing the paints. It is, however, well known that mixtures of pure linseed oil with any red lead containing more than, say, 5 per cent of litharge will rapidly harden; not merely settle, but apparently combine with some of the oil. Nothing is better known about red lead than this.

Mr. Gardner's remark that some action may take place other than the formation of lead linoleate is worthy of consideration; also his variable results on the solution of lead in the supernatant oil. There are many unsettled problems in this difficult subject.

It is generally true that with a given pigment the finer it is the more oil it takes; but there is an apparent exception in the case of red lead. Here the coarser and slightly heavier sorts take most oil, because they are chemically active towards oil

Mr. Sabin. and become viscous more rapidly than the finer, which are more completely oxidized. However, the exception is apparent rather than real, because the high litharge content makes it a different pigment. If we are ever able to make coarse red lead of 100 per cent Pb_3O_4, we may find that the general rule holds. But the experience of the present writer is that there should have been no occasion for using more oil with 99 per cent red lead than with 98 per cent; and the observation of the writer and his associates has extended over several million pounds of these mixtures.

The same thing may be said about the weather-resisting quality. Mr. Gardner finds this to increase in exact proportion to the amount of litharge present; this is precisely contrary to the results obtained by Committee D-1 on the Havre de Grace bridge; the experience of Chief Engineer Wagner, of the Philadelphia and Reading Ry. Co., for nineteen years, as described in the Transactions of the American Society of Civil Engineers[1]; that of the U. S. Navy Department, which has for many years limited the amount of litharge to 6 per cent and which last year issued specifications for 97-per-cent red lead, based on three years' experience and the use of more than a million pounds of material; and that 97 or 98-per-cent red lead is almost exclusively used by the U. S. Engineering Corps; also that it is specified on 33,000 miles of railway, and extensively used by 65,000 miles more. These facts it is pertinent to cite in opposition to Mr. Gardner's isolated tests.

The best real test of a preservative coating is its use on a working scale; and these facts show that the use of red lead containing a minimum amount of litharge is rapidly displacing the lower-grade product; and this widely distributed use can only be based on experience. Mr. Gardner thinks his experiments show that large litharge content is important; I maintain that not only do the tests of Committee D-1 show the opposite, but, what is still more important, this conclusion is contrary to the opinion, over the whole country, of the users of great quantities of the highest obtainable percentage of Pb_3O_4, at prices higher than ever before known, and by consumers interested only in efficiency. This is the final test.

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[1] Vol. LXXVII, p. 963.

MR. G. W. THOMPSON.—I just want to call attention to one little inaccuracy; I think it was unintentional on the part of Mr. Gardner. In referring to the amount of red lead, dissolved in the vehicle, I presume he means ash or other lead compounds dissolved in oil. He stated that these figures represented the percentage of the red lead which was dissolved. He evidently did not mean this, because the table showed the contrary. I think that attention should be called to this, because, considering that the oil is the smaller percentage of the two components in these paints, you could probably take those figures and divide by 4 or in some cases 5 and get a more accurate representation of the percentage of pigment which entered into solution in the oil. **Mr. Thompson.**

MR. H. J. FORCE.—About three years ago we had occasion to investigate the question of red lead. Our company, the Lackawanna Railroad, were large users of red lead, especially for bridge work. We accordingly procured through our purchasing agent, in open market, four samples of red lead. These samples were properly prepared in the laboratory to be about the proper consistency for painting, all samples being thoroughly ground up with linseed oil in a wedgewood mortar and applied to steel panels 12 x 18 in., in duplicate; the first coat being allowed to dry 48 hours, and then given the second coat, the plates were exposed on angle of 45 deg. When I received the preprint of Mr. Gardner's paper, I was very much interested and immediately sought to make another inspection. The following is the rating of the four samples of red lead. Lead No. 1, 98 per cent of red lead, had a rating at 18 months of 10, and at three years of 9. Lead No. 2, which contained 93 per cent of red lead, had a rating at 18 months of 9, and at 3 years of 9. Lead No. 3, which contained 83 per cent of red lead, was given a rating of 9 at 18 months, and 9 at 3 years. We did notice, however, that there was a slight difference in the chalking. On these three samples, so far as we are able to determine, the rating seems to be one that is consistent at this time. The fourth sample contained 18 per cent red lead, and over 70 per cent of barium sulfate. This paint took on a color which was quite yellow inside of 7 or 8 months, but at the present time it is showing up very satisfactorily. The rating of this paint at 18 months was 7, and at the end of 3 years it was 7. **Mr. Force.**

Mr. Force. I want it understood that I am making no prediction as to what the results will be at the end of 5 years. Another inspection in a year from now may show results entirely different from that which I have already reported. Our company has purchased a considerable amount of red lead, and up to the present time it is giving us excellent service. As to whether or not we are justified in buying a higher grade of red lead, we believe our tests will show in course of time.

Mr. Carpenter. MR. A. W. CARPENTER.—Some years ago I conducted a somewhat similar experiment to that which Mr. Force has mentioned, for the engineering department of the New York Central Railroad. We obtained samples of red leads ranging from about 81 per cent true red-lead content up to 94, which was the highest, I believe. There were five samples of different manufacturers. No sample was ground in oil. We could not determine much difference in the durability of the various coats, so that we came to the conclusion that the litharge content was not so important as it was being represented to be, within the limits of our experiment.

Mr. Cheesman. MR. F. P. CHEESMAN.—As none of the members have criticized the specimen plates of iron-oxide paints which apparently have weathered equally as well as the best plates of red-lead paint, and taking into consideration the fact that a first-quality iron-oxide paint costs to-day about one-third the price of a first-quality red-lead paint; and also remembering that an iron-oxide paint will cover about 20 per cent more surface than a red-lead paint, and that the labor cost of applying a red-lead paint is about twice the cost of applying an oxide paint, so that all things considered, a red-lead paint to-day will cost approximately four times as much as an iron-oxide paint: Would it not be well to call the attention of the consumer to these facts, so that he may in these days of high prices use an iron-oxide paint and save about $75 out of each $100?

Mr. Chapman. MR. CLOYD M. CHAPMAN.—I should like to ask a question regarding the effect of chromating upon the solubility of red lead in oil. The solubility of the seven pigments used as indicated by the ash varied from 0.31 to 15.82 per cent as shown in Table III. The chromated red lead gave 0.80 per cent ash. Which of the seven red leads was used for chromating? Was it one which gave a relatively high or a low percentage of ash before being chromated? Was its solubility materially increased or decreased by the chromating process? **Mr. Chapman.**

MR. J. A. SCHAEFFER.—If I understand Mr. Sabin correctly, I want to take exception to one point which he has raised in his paper, namely, that there is not a great difference in the proportion of oil which is required for red leads of varying fineness, therein differing from other pigments. This has not been our experience. Red-lead pigments follow the same course as other pigments in relation to the amount of oil required with varying fineness. I know of a number of red leads in which we find a variation in the percentage of fine particles which, if all were used with the same proportion of oil, would give greatly different consistencies in the finished pastes. I can show red leads of such a degree of fineness that a paint of proper painting consistency is obtained from the use of as low as 14 lb. of pigment to a gallon of oil; whereas if the usual quantity of pigment, 25 lb., were used to the gallon of oil, a paint would be obtained of such a stiff consistency that it could not be applied. This is not due to a difference in the red lead content, as the same consistency will be obtained with the red lead content varying from 70 to 98 per cent. On comparing these finer pigments with the coarser ones, in the case of red lead, I cannot agree with Mr. Sabin that it is directly dependent on the peculiar action which red lead has with oil. The red-lead pigments, so far as I have studied them, do not vary to a very great extent from the general nature of other pigments when ground in oil, though they do show a slightly different action on prolonged standing. **Mr. Schaeffer.**

THE CHAIRMAN (MR. S. S. VOORHEES).—The point which was brought out by Mr. Schaeffer is extremely interesting as measuring spreading consistency. The behavior of different paints when mixed with oil to determine their viscosity, as we term it, for want of a better word, has been one of the problems of Committee D-1 in endeavoring to determine the proper consistency of a paint and how it can be measured in some reproducible manner. In certain of the paint tests which have been conducted a form of viscosimeter was used; and while the same viscosity, or consistency, as measured by the Stormer **The Chairman.**

The Chairman. viscosimeter was obtained, the behavior of paints under the brush was not alike. The point brought out by Mr. Schaeffer in connection with Mr. Sabin's discussion on the different behaviors of red lead with oil will require further investigation in order to arrive at the fundamental facts.

Mr. Thompson. MR. THOMPSON.—May I say another word? I have had a great deal of experience with red lead; for twenty-five years I have been giving it thought and study. Twenty-five years ago the red lead made and sold for painting purposes was nothing like the red lead which is made to-day. There was very little red lead made then that contained much more than 80 per cent true red lead, excepting of course orange minerals. The red leads were very coarse, giving perhaps 2 per cent on No. 19 silk cloth and 30 per cent of "sandy" lead. Now it has been my firm conviction during all of these years that what is true with regard to other pigments should also be true with regard to red lead, and that if coarseness is a bad thing in the case of white-lead or iron-oxide pigments, for instance, it should be a bad thing in red lead. I have endeavored, so far as my sphere of action went, to secure the manufacture of finer red leads, and in this time we have seen generally throughout the country an improved standard, that is, all manufacturers have been making red leads containing higher percentages of true red lead. What is more, with this higher percentage of true red lead there has also appeared a greater degree of fineness, which is an improvement from the pigment standpoint. Now having this in mind and realizing that about ten years ago, we will say, it was quite general that a specification should call for 94 per cent of true red lead, it seemed possible that a red lead could be produced that would approach nearly 100 per cent red lead, if sufficient fineness was given to the pigment; and that apparently is what has taken place. Whether I have had any part in this progress of red lead or not, here is the fact. I asked a member of Committee D-1 two or three years ago to send me samples of red lead which came to him from different parts of the country. I did not know their identity at all. As some of you know, I read a paper before the Society six or seven years ago[1] on a classification apparatus in which

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[1] "The Classification of Fine Particles According to Size," *Proceedings*, Am. Soc. Test. Mats., Vol. X, p. 601 (1910).

Mr. Thompson. the particles of a pigment are grouped by flotation into different groups. The first group is called the sandy portion. The whole pigment is first put through a bolting cloth, obtaining what is called a "coarse" portion, and then the first group by flotation is the "sandy" portion. In every case with those samples, where the percentage of litharge was high they showed a high percentage of sandy lead, and where the litharge content was small they showed a small percentage of sandy lead.

Now I am firmly convinced of this fact, that no paint pigment should contain more than 3 per cent of sandy particles, I don't care what it is for, if it is going to be mixed with oil. Sometimes it is hard to control manufacture so that it will not contain more. I will say further that a red lead that runs 98 per cent of true red lead will never contain over 3 per cent of sandy lead. Now this is the real practical question in connection with the red lead question. The painter is a practical man applying a brush of paint. One of the primary considerations which he must take thought of is the workability of the paint, and it is a fact that a paint with a high percentage of true red lead is a better, more workable paint—not because it contains a high percentage of true red lead, but because it is finer and gives a better film.

Now as to this question of the oil, 98-per-cent true red lead can be mixed to a painting consistency with less oil than any other low oxidized red lead. The pigment content can be worked up; and if there is one thing I have been thoroughly convinced of from the days of Doctor Dudley to now, it is that to get a permanent paint the binder should be cut down as much as possible within practical limits and the proportion of pigment increased. Now that can be done with a highly oxidized red lead where it cannot be done with a low oxidized red lead.

Mr. Coleman. MR. R. E. COLEMAN.—There were some remarks made by Mr. Sabin relative to the volumes of the different paints and their percentages of pigment and vehicle which I believe deserve more consideration than Mr. Sabin brought out in his remarks. Some time ago many reports were published purporting to show that the bulking figure of a pigment in oil was in direct ratio to its specific gravity. This was suggested as

Mr. Coleman. a method for determining the volume that any pigment would give in oil. I got from Mr. Sabin's discussion that this was the point conveyed. If we look at it in a purely superficial manner, it is liable to show this; but during my experience of over twelve years in factory efficiency work, extensive investigations have been made in order to determine why we did not always get as much paint out of a batch as we should from a calculation of volumes based on the above method. The great variance of results has led me to believe that there are other factors which will affect these volumes a great deal. If we would confine ourselves entirely to the question of a simple mixture of pigment and oil, no doubt it would hold good, but I believe there are present so many other factors which we are now beginning to realize, that a definite volume ratio does not always hold good. Therefore we must accept the statement that these things are merely mathematical calculations with a great deal of reserve, until we prove them out in some definite manner. It has been my experience that this method does not always check up. I have examined several thousand formulas in my experience with this in mind, and have also had others remark to me regarding the variations in volumes that obtained with different pigment and vehicle combinations.

Mr. Gardner. MR. H. A. GARDNER.—Mr. Sabin's discussion is based on what I believe to be unwarranted theoretical refinements. It is true that a standard formula of 25 lb. of dry pigment to one gallon of oil was selected in making these paints. However, in the grinding there are always some losses due to evaporation, wetting of stones, and other factors which are well known to the practical factory paint maker. Also in the taking of samples for analysis there may be some slight differences which cannot always be accounted for. Moreover, it is stated in my paper that "the red lead paints were prepared so that they would all be practically of the same viscosity or body," that is to say, the quantity of oil was varied, where necessary, with that in view.

Because of these circumstances, it was thought desirable to determine the actual compositions of the paints as used. They were therefore analyzed and the percentages by weight of pigment and oil, thus determined, are given in Table I.

Mr. Gardner.

Mr. Sabin's assumption that the specific gravity (or density) of the red lead used in my paints is 8.7 is unwarranted. The specific gravities of the red leads used in my tests varied. Moreover, in my opinion, calculations of paint volumes and weights based upon the assumed specific gravity of the pigment—in this instance red lead—cannot properly be made the basis of criticism of actual analytical results. In that connection, the statement in Mr. Coleman's discussion, that the volume of a mixture of pigment with vehicle cannot always be calculated from the specific gravity of the pigment, is pertinent.

Answering Mr. Thompson's inquiry as to the ash test, I should like to say that these tests were determinations of the actual amount of pigment in solution as determined by ashing.

Answering Mr. Chapman's point, I wish to state that the type of red lead used for chromatizing was No. 2.

Answering Mr. Thompson's statement that red lead requiring the lowest amount of oil is the best red lead, I would refer to the old statement, which I believe was made by Doctor Dudley, to the effect that "oil is the life of paint," and a pigment requiring a large amount of oil is often the most durable.

In respect to the Havre de Grace tests, I might state that two grades used, namely 85 and 94 per cent, were made into paints with entirely different oils and on different percentages, and there cannot therefore be any consistent results obtained with such work.

Further answering Mr. Thompson's statement regarding the use of red lead by the Navy Department, it is perfectly true that they use large amounts of 94 and 98 per cent; but I would refer to the fact that at the Navy Yard in San Francisco excellent results have been obtained with the 85-per-cent red lead which they have used, and I believe over two million pounds have been consumed at that point.

Mr. Sabin.

MR. SABIN (*by letter*).—Mr. Schaeffer's statement that with red lead of extreme fineness nearly twice as much oil is needed as with the ordinary sort should, I think, be verified and exact data published, as Mr. Voorhees suggests. It is contrary to all experience known to the writer; and I suggest that samples be sent to the Bureau of Standards, or to the chairman of the Sub-Committee of Committee D-1 on Physical Tests, for experi-

Mr. Sabin. ment. Mr. Schaeffer's further statement that red lead behaves much like other pigments in oil is notoriously opposed to all popular belief, as it is to the specific statements of Mr. Thompson and the writer.

The supposition advanced by Mr. Coleman that the mixture of a pigment with a vehicle gives a volume which cannot be calculated from their specific gravities is startling. It is well known that the method of finding the specific gravity of a pigment is by dividing the weight of a portion of it by that of the volume of water which it displaces; or if some other liquid is used the density is calculated to that of water. In one laboratory with which I am familiar this experiment is usually made with kerosene, or some other petroleum product; in another it is the practice to use water. Specific gravity tables made by these two methods agree exactly, as they should if the pigments are sensibly insoluble in both liquids; but Mr. Coleman's contention is equivalent to saying that in the liquids (chiefly oil) used in paints the specific gravity of a pigment varies from this figure. This is important if true. I am prepared to say that it is not true of red or white lead in linseed oil; and for many years I was chemist for a factory making varnish paints and paints in japan, and within the limits of error, which could not have exceeded 0.5 per cent, the volumes of pigments and these complex and numerous vehicles were additive. If Mr. Coleman can describe some definite experiment proving his contention, I am sure Committee D-1 will verify it if possible.

DETERMINATION OF ABSOLUTE VISCOSITY BY THE SAYBOLT UNIVERSAL AND ENGLER VISCOSIMETERS.

By Winslow H. Herschel.

SUMMARY.

The commonly accepted formula for determining absolute viscosity by the capillary tube method is

$$\text{Kinematic Viscosity} = \frac{\text{Absolute Viscosity}}{\text{Density}} = At - \frac{B}{t}$$

in which t is the time of discharge, in seconds, and A and B are instrumental constants which may be calculated from the equation when the times of discharge, for two liquids of known kinematic viscosity, have been experimentally determined. This investigation of Engler and Saybolt Universal viscosimeters has shown that the critical velocity at which the equation ceases to hold good, is equal to about 800 times the kinematic viscosity, divided by the diameter of the tube. This value is exceeded with water, so that water can not be used as one of the two liquids. Consequently all equations are in error which have been based upon tests with water, including the much-used equation of Ubbelohde for the Engler viscosimeter.

DETERMINATION OF ABSOLUTE VISCOSITY BY THE SAYBOLT UNIVERSAL AND ENGLER VISCOSIMETERS.

By Winslow H. Herschel.

It is becoming generally recognized that it is desirable to express viscosity in absolute units.[1] Where viscosity is used merely for the purpose of identification, the time of discharge of a certain viscosimeter serves as a fairly satisfactory method of expressing viscosity, the chief disadvantage being that numerous conversion tables are necessary for intercomparisons between the various instruments in use. When, however, viscosity is used to calculate the frictional resistance of a bearing, or the load which it will carry with safety, it becomes evident that viscosity must be expressed in some unit which is independent of the measuring instrument. Such a unit is the absolute or c. g. s. unit, which, following the suggestion of Deeley and Parr[2] will be called a "poise." It should be noted that the poise is just as useful as the time of discharge for purposes of identification.

If it is assumed that the viscosimeters now commonly employed will continue in use, even if it becomes the general practice to report viscosities in poises, then it is desirable to devise means for obtaining the viscosity in poises from the time of discharge.

Several equations have been used or proposed for finding the viscosity in poises from the time of discharge of the Engler, Redwood and Saybolt Universal viscosimeters, and for convenience of reference they are given in Table I.

It will be seen that all the equations of Table I are of the form

$$\frac{\mu}{\gamma} = At - \frac{B}{t} \qquad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

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[1] G. E. Upton and A. E. Flowers, *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part I, pp. 296, 317, respectively (1915); P. C. McIlhiney, *Journal of Industrial and Engineering Chemistry*, Vol. 8, p. 433 (1916).

[2] R. M. Deeley and P. H. Parr, *Philosophical Magazine*, Vol. 26, p. 87 (1913).

where μ is the viscosity in poises, γ is the density in grams per cubic centimeter; t is the time of discharge in seconds, and A and B are instrumental constants to be found either by theory or experiment. The experimental determination of these

TABLE I.—EQUATIONS FOR DETERMINING ABSOLUTE VISCOSITY FROM TIME OF DISCHARGE.

Authority.	Equation.
ENGLER VISCOSIMETER.	
Higgins,[1] calculated from Ubbelohde's equations[2]	$\frac{\mu}{\gamma} = 0.001435\,t - \frac{3.22}{t}$
Meissner[3]	$\frac{\mu}{\gamma} = 0.0015723\,t - \frac{3.5767}{t}$
Upton, from graphical method	$\frac{\mu}{\gamma} = 0.001432\,t - \frac{3.216}{t}$
REDWOOD VISCOSIMETER.	
Higgins	$\frac{\mu}{\gamma} = 0.00260\,t - \frac{1.715}{t}$
Herschel, calculated from Meissner's equations[4]	$\frac{\mu}{\gamma} = 0.00260\,t - \frac{1.561}{t}$
Flowers, calculated from Meissner's data	$\frac{\mu}{\gamma} = 0.002631\,t - \frac{1.5770}{t}$
Upton, from graphical method	$\frac{\mu}{\gamma} = 0.002322\,t - \frac{1.387}{t}$
SAYBOLT UNIVERSAL VISCOSIMETER.	
Flowers, calculated from Meissner's equations	$\frac{\mu}{\gamma} = 0.0021863\,t - \frac{1.4968}{t}$
Upton, from graphical method	$\frac{\mu}{\gamma} = 0.001930\,t - \frac{1.242}{t}$

[1] W. F. Higgins, *Collected Researches*, The National Physical Laboratory, Vol. 11, p. 14 (1914).
[2] L. Ubbelohde, *Tabellen Zum Englerschen Viskosimeter*, pp. 9, 27 (1907).
[3] W. Meissner, *Chemische Revue über die Fett- und Harz-Industrie*, Vol. 17, p. 202 (1910).
[4] W. Meissner, *Chemische Revue über die Fett- und Harz-Industrie*, Vol. 19, p. 30 (1912).

constants will be called the calibration of an instrument. The viscosity divided by the density, $\frac{\mu}{\gamma}$, is known as the kinematic viscosity, and it is evident that the absolute viscosity, μ, can not be found from Eq. 1 until the density has been determined, by means of a hydrometer or otherwise.

Bingham's equation[1] for determining viscosity by the capillary-tube method reduces to Eq. 1, and may also be written

$$\frac{\mu}{\gamma}=\frac{\pi g d^4 t}{128\,Q\,(l+\lambda)}\left(h-\frac{m\,v^2}{g}\right)\ldots\ldots\ldots\ldots\quad(2)$$

where d and l are the diameter and length, respectively, of the outlet tube, in centimeters; Q is the volume, in cubic centimeters, discharged in the time t, in seconds; λ is a correction to be added to the measured length to give the effective length of the tube; and m is the coefficient of the kinetic energy correction, $\frac{m\,v^2}{g}$.

As pointed out by Bingham, Schleisinger and Coleman,[2] Eq. 2 will hold good for a long capillary tube if λ is neglected, m is taken as 1.12 after Boussinesq,[3] and the average head, h, is determined by Meissner's equation

$$h=\frac{h_1-h_2}{\log_e\left(\frac{h_1}{h_2}\right)}\ldots\ldots\ldots\ldots\ldots\ldots\quad(3)$$

where h_1 and h_2 are the initial and final heads, respectively. The kinetic energy correction was neglected in deriving Eq. 3. Unfortunately, technical viscosimeters are supplied with very short tubes, as shown by Table II, and there is need of further investigation.

Before Eq. 2 can be applied to technical viscosimeters, it is necessary that normal standard values, with suitable tolerances, should be available for the principal dimensions. These values are given in Table II for the Engler viscosimeter, but have not as yet been adopted for other instruments. It is also necessary to know what are the most probable values of λ and m, the effect of error in Eq. 3, and the velocity at which turbulent flow begins.

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[1] E. C. Bingham, U. S. Bureau of Standards, *Scientific Paper No. 278*, p. 319 (1916). For history of the development of this formula see G. H. Knibbs, *Journal* and *Proceedings*, Royal Society of New South Wales, Vol. 29, p. 105 (1895).

[2] E. C. Bingham, H. I. Schleisinger and A. B. Coleman, *Journal*, Am. Chem. Soc., Vol. 38, p. 27 (1916).

[3] J. Boussinesq, *Comptes Rendus*, Vol. 113. pp. 9 to 15 and 49 (1891).

Table II.—Dimensions of Saybolt Universal and Engler Viscosimeters, in Centimeters.

Saybolt Universal Viscosimeter.

Dimensions.	According to	
	Meissner.	Gill.[1]
Mean diameter of outlet tube, cm	0.178	0.180
Length of outlet tube, cm	1.41	1.30
Diameter of container, cm	2.968	3.000
Head on lower end of tube, at start, cm	12.688	12.60
Head on lower end of tube, at end, cm	4.016	......
Average head, cm	7.538	......
Capacity of container, cc	......	70

[1] A. H. Gill, "Oil Analysis," p. 29 (1913).

Engler Viscosimeter.

Dimensions.	According to		
	Bureau of Standards No. 2204 U.	Ubbelohde.	
		Normal.	Allowable Variation.
Mean diameter of outlet tube, top, cm	0.2925	0.290	0.002
Mean diameter of outlet tube, bottom, cm	0.2829	0.280	0.002
Length of outlet tube, cm	2.00 or 1.97	2.00	0.01
Diameter of container, cm	10.584	10.60	0.1
Head on lower end of tube, at start, cm	5.184	5.200	0.05
Head on lower end of tube, at end, cm	2.911	2.934[a]	.....
Average head, cm	3.939	2.959[a]	.....
Capacity of container, cc	247	240[b]	4[b]

[a] According to Meissner.

[b] According to Holde: D. Holde, "Untersuchung der Mineralöle und Fette," p. 102 (1905). By agreement between German laboratories, this requirement was abandoned April 1, 1907. See *Chem. Ztg.*, Vol. 31, p. 447 (1907).

Reynolds' Criterion.

It has been found by Reynolds[1] that the critical velocity, at which turbulence begins, is given by the equation

$$v = \frac{\mu}{d\gamma} 2000, \quad \text{or}$$

$$\frac{v d\gamma}{\mu} = 2000 \quad \ldots\ldots\ldots\ldots\ldots \quad (4)$$

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[1] O. Reynolds, *Philosophical Transactions*, Royal Society, Vol. 174, Part 3, p. 948 (1883).

The ratio $\frac{v\,d\,\gamma}{\mu}$, which is the product of the velocity and diameter divided by the kinematic viscosity, is known as Reynolds' criterion. His value of 2000 was obtained from experiments made with long pipes, and there is nothing in his work to refute the various other values found with shorter tubes[1] by other experimenters. It has been pointed out by Flowers[2] and by Hayes and Lewis [3] that the rate of flow of water at 20° C. (68° F.) through the tubes of technical viscosimeters is such that the critical velocity is exceeded, although the value of Reynolds' criterion for this case is less than 2000 if the average velocity is used in Eq. 4. The velocity decreases greatly as the head decreases, but the average velocity has been used in all calculations.

A NEW FORM OF DIAGRAM.[4]

As a help in studying the flow in short tubes, a new diagram has been devised in which Reynolds' criterion is plotted against the percentage error in Poiseuille's formula,[5]

$$\mu' = \frac{\gamma\,\pi\,g\,d^4\,h\,t}{128\,Q\,l} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

which results from Eq. 2 when λ and the kinetic energy correction are negligible. It is evident that the percentage error in Eq. 5 is equal to $100\left(\frac{\mu'}{\mu} - 1\right)$. Values of the true viscosity, μ, for water, alcohol and water, and sugar solutions, have been obtained with long-tube viscosimeters[6] and these liquids may be used for calibrating short-tube instruments, the percentage error for different values of Reynolds' criterion being obtained by varying the viscosity, instead of varying the pressure, as with a long-tube instrument.

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[1] For table of theoretical and experimentally determined values see L. V. King, *Philosophical Magazine*, Vol. 31, p. 338 (1916).

[2] A. E. Flowers, *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part II, p. 577 (1914).

[3] H. C. Hayes and G. W. Lewis, *Journal*, Am. Soc. Mech. Engrs., Vol. 38, p. 629 (1916).

[4] See *Journal*, Washington Academy of Sciences, Vol. 6, p. 155 (Mar. 19, 1916).

[5] J. L. M. Poiseuille, *Mémoires de l'institut, savants étrangers*, Vol. 9, pp. 433 to 544 (1846); *Comptes Rendus*, Vol. 2, p. 961 and 1041 (1840), Vol. 12, p. 112 (1841).

[6] E. C. Bingham and R. F. Jackson, U. S. Bureau of Standards, *Scientific Paper No. 298*.

It may be shown mathematically that if, in a diagram like Fig. 1, θ is the angle between the calibration curve for a given tube, and the horizontal, then

$$\tan \theta = \frac{32 l}{100\, m\, d} \quad \ldots\ldots\ldots\ldots\ldots \quad (6)$$

and the value of $\frac{\mu'}{\mu}$ at the point where the calibration curve, extended, intersects the axis of abscissas, is equal to

$$\frac{\mu'}{\mu} = \frac{l+\lambda}{l} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7)$$

so that the percentage error at this point will be $\frac{100\,\lambda}{l}$.

APPLICATIONS OF THE NEW DIAGRAM.

The Value of Reynolds' Criterion at the Critical Velocity.—Fig. 1 has been constructed from data of Poiseuille and Hosking[1] and tests made by the writer at the Bureau of Standards.[2] It shows that for a given tube, the error in Eq. 5 increases with the value of Reynolds' criterion, and for a given value of the criterion, the error increases as the ratio of length to diameter decreases. That the error is a function of Reynolds' criterion and $\frac{l}{d}$ has been pointed out by Grüneisen.[3]

Reynolds and others have noted that there is an intermediate, unstable régime, between the regions of viscous and of turbulent flow, so that concordant results are difficult to obtain at velocities slightly above the critical velocity. Doubtless the scattering of the points in the upper part of Fig. 1 is to some extent due to this difficulty. It is possible, however, by taking points closely together, to follow fairly accurately the change from one stable régime to the other.[4] Fig. 1 shows that with Hosking's long tube, the marked change in direction in the calibration curve, which indicates the critical velocity, occurs at a value of Reynolds' criterion somewhat greater than 2000.

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[1] R. Hosking, *Philosophical Magazine*, Vol. 18, p. 260 (1909).

[2] A more detailed account of these experiments will be published later as U. S. Bureau of Standards, Technologic Paper No. 100.

[3] E. Grüneisen, *Wissenschaftliche Abhandlungen*, Die physikalisch technischen Reichsanstalt, Vol. 4, Part 2, p. 151 (1905).

[4] See T. E. Stanton and J. R. Pannell, *Collected Researches*, The National Physical Laboratory, Vol. 11, p. 302 (1914).

With the curves for the short tubes of the Saybolt Universal and Engler viscosimeters, the bend occurs at a much lower velocity, and in the opposite direction. It might therefore be expected that there would be no indication of a critical velocity to be observed on the calibration curves for tubes of intermediate length.

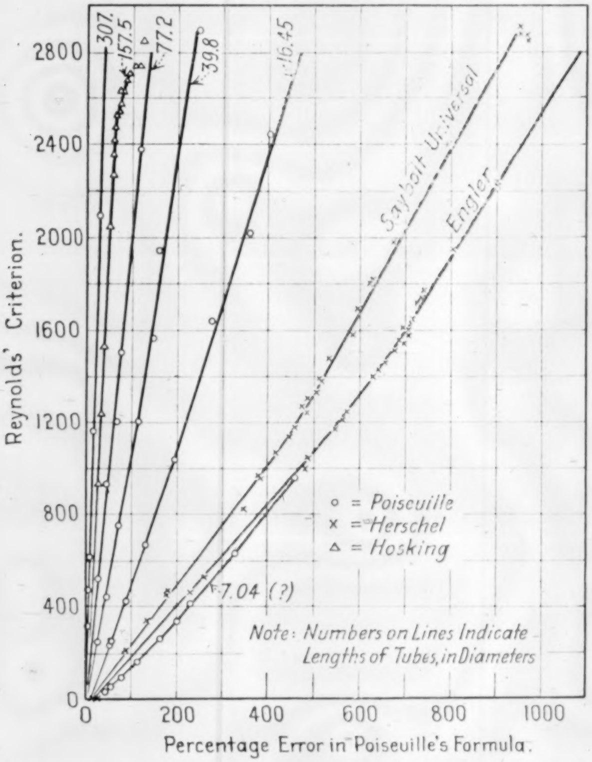

FIG. 1.—Tests with Capillary Tubes and Viscosimeters showing Values of Reynolds' Criterion at the Critical Velocity.

The bend in the calibration curve is also plainly seen in Fig. 2, which is constructed according to the method used by Higgins[1] and by Upton.[2] Greater weight was given to points obtained with alcohol than to the two obtained with sugar solution. Sugar solutions change very rapidly in viscosity as the water evaporates, while alcohol solutions of approximately 50-per-cent alcohol change very little in viscosity with a change in concentration.

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[1] W. F. Higgins, *Collected Researches*, The National Physical Laboratory, Vol. 11, p. 13 (1914).

[2] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part I, p. 299 (1915).

Fig. 3 shows the lower part of Fig. 1 to an enlarged scale. The slopes of the lines *A* to *E*, *G*, *I*, *L* and *M* correspond to the value 1.12 for *m* in Eq. 6, but the points of intersection of these lines with the axis of abscissas are located from the experimental data. The lines *F*, *H*, *J* and *K* are experimentally determined calibration curves. It will be noticed that none of the points for the Engler viscosimeter lie above the line *J*, for values of Reynolds' criterion less than 800. This indicates that a change of régime begins at about this value, with tubes 7 or 8 diameters in length.

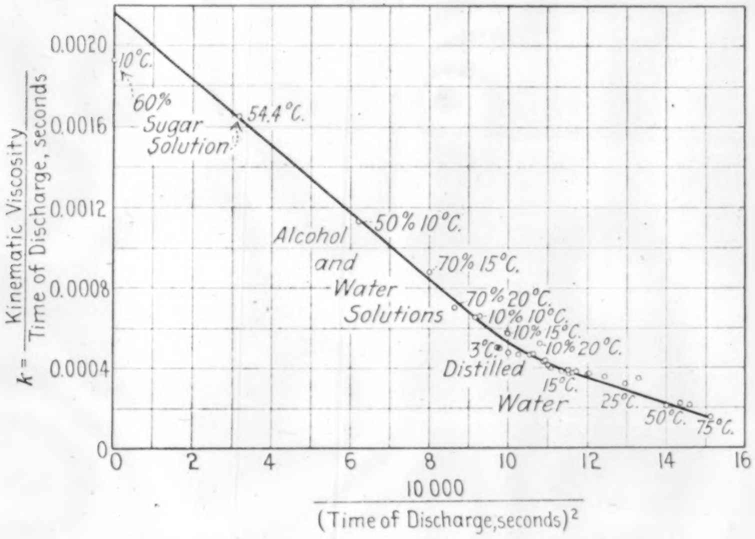

FIG. 2.—Calibration Curve of Saybolt Universal Viscosimeter by Higgins' Method.

The velocity of outflow of water at 20° C. (68° F.) from the Engler viscosimeter is such that the value of Reynolds' criterion is about 1750, the corresponding value for the Saybolt viscosimeter being about 1500. Consequently water is not a suitable liquid for determining the constants of Eq. 1, or even for standardization as now required for the Engler viscosimeter, and all equations of Table I must be in error, since the change of régime at the critical velocity has been ignored in their derivation.

The Correction, λ, *to the Measured Length of Tube.*—According to Eq. 7, when the calibration curve of a given tube cuts the axis of abscissas to the right of the origin (the abscissas representing the percentage error), the value of λ must be positive. Bearing this in mind, it may be seen from Figs. 1 and 3, that a small and positive amount must be added to the measured length of a tube to get the effective length. This is in agreement with Couette[1] but not with Knibbs. The latter found that λ could be either positive or negative, but his results are based on the unproved assumption that λ is equal to *n* times the radius of the tube, and that *n* has the same value for a series of tubes of the same diameter but of different lengths.

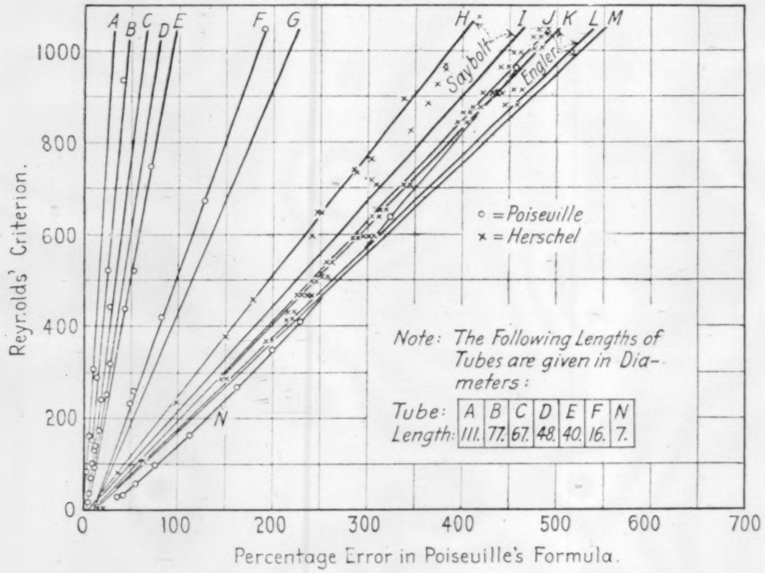

Fig. 3.—Tests with Capillary Tubes and Viscosimeters showing Method of Determining λ and *m*.

Poiseuille expressed a doubt in regard to the length of his shortest tube, and the value stated, 1 mm., is probably inaccurate, as shown by the excessively high value of λ or of n indicated on Figs. 1 and 3. Omitting this tube, values of n for other tubes used by Poiseuille were obtained by Eq. 7, and are as follows:

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[1] M. Couette, *Annales de chimie et de physique*, Vol. 21, p. 494 (1890).

LENGTH OF TUBE, DIAMETERS.	VALUE OF n.
111	6.5
77.2	7.8
67.4	5.4
47.8	4.7
39.8	0.0
16.45	3.3

These values show that n is probably variable, and that Higgins' value of 1.64 for all tubes does not apply. His value is based on a theory which assumes submerged discharge and neglects the effect of surface tension.

The Coefficient, m, of the Kinetic Energy Correction.—Since the slopes of the lines A to E of Fig. 3 agree with the experimentally determined points, and also with the theoretical value, 1.12, of the coefficient of the kinetic energy correction, it is probable that this is close to the correct value.

Line H is the calibration curve for normal filling, and it was found that when abnormal volumes were used, so as to make the head more constant, the points were nearer line I. That the slope of the calibration curve approached the theoretical, as the head became more constant, was more plainly seen in the case of the Engler instrument, with which a much greater number of tests were made with abnormal volumes put in and discharged. In the report on the dimensions of the Engler instrument it was stated:

"The extreme length of the capillary tube was found to be 20.0 mm. ±0.1 mm., while the length of the capillary measured inside between the points at the ends of the tube where the rounding begins, was found to be 19.70 mm. ±0.05 mm."

This explains the two pairs of lines on Fig. 3, J-K and L-M, J and L being calculated for an assumed length of tube of 2.00 cm., and K and M being based on a length of 1.97 cm.

It is evident that the more nearly constant the head, the less would be the error in Eq. 3 due to the neglect of the kinetic energy correction in its derivation. Hence it may be concluded that Boussinesq's value of 1.12 for the coefficient of the kinetic energy correction would be approximately applicable, even for short tubes. If the average head is determined by Meissner's formula, which is the best available, a lower value must be used for the coefficient. It seems possible that the discrepancy between lines F and G in Fig. 3 is due to error in determining the average head.

New Equations for the Engler and Saybolt Universal Viscosimeters.—From lines J, K and H of Fig. 3, it was found that n had a value of 0.8 for the Saybolt instrument and 1.4 for the Engler, while m was 0.97 for the former and 1.03 for the latter. Using these values, the equations for obtaining kinematic viscosity from the time of discharge were found to be

$$\frac{\mu}{\gamma}=0.00213\,t-\frac{1.535}{t}\ \text{(Saybolt Nos. 580 and 727)}\ldots\quad(8)$$

$$\frac{\mu}{\gamma}=0.00147\,t-\frac{3.74}{t}\ \text{(Engler No. 2204 U)}\ldots\ldots\quad(9)$$

These equations should not be used for values of t less than 31 seconds, Saybolt, or 56 seconds, Engler. Comparison of Eqs. 1 and 9 with the equations of Table I, shows that there is a fair agreement between Eq. 9 and the equations of Upton and Ubbelohde in regard to the value of A, or, what is the same thing, in regard to n. Meissner's equation is in error because he takes n equal to zero. Ubbelohde and Upton are, however, in error in regard to the value of B or m. The greatest error in Ubbelohde's equation will be for a liquid having a viscosity of about 0.015 poise, as for 10-per-cent alcohol at 20° C. (68° F.), or for a value of Reynolds' criterion of about 1000, and this error will amount to about 20 per cent. In the case of the equations for the Redwood and Saybolt viscosimeters, there were the additional causes of error that different authorities used different experimental data, or different instruments, with unequal dimensions.

The dimensions of Engler viscosimeter No. 2204 U, as given in Table II, were used to calculate the water rate, or time of discharge for water at 20° C. (68° F.), by Meissner's method. This gave a value of 50.3 seconds, while the water rate found by direct experiment was 51.3 seconds. Since one value is below, and the other is above the normal value of 51 seconds, it seems futile to attempt the correction of Eq. 9 to make it more accurate for an instrument of normal dimensions. The correction of Eq. 8 is also impossible until standard dimensions have been adopted for the Saybolt instrument.

Further experiments with Engler viscosimeters, and with Saybolt viscosimeters of standard dimensions, would be necessary to fix the limits within which the constants of Eqs. 8 and 9 might be expected to vary.

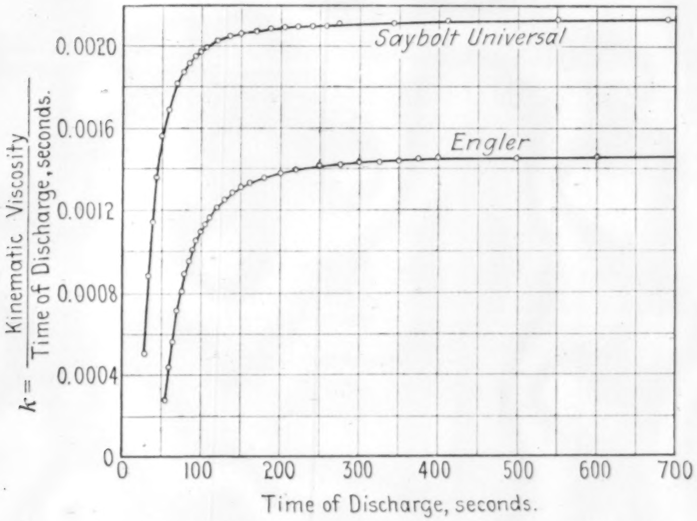

FIG. 4.—Values of the Kinematic Viscosity divided by the Time of Discharge, for the Saybolt Universal and Engler Viscosimeters; Archbutt and Deeley's Method.

APPLICATIONS OF EQUATIONS 8 AND 9.

Archbutt and Deeley[1] use the equation

$$\mu = k t \gamma \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (10)$$

where k is a variable instrumental value to be determined by calibration with liquids of known viscosity and density. From Eqs. 1 and 10,

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[1] L. Archbutt and R. M. Deeley, "Lubrication and Lubricants," p. 180 (1912).

$$k = A - \frac{B}{t^2} \qquad (11)$$

so that k may be calculated for any assumed values of t; A and B being taken from an equation of the form of Eq. 1.

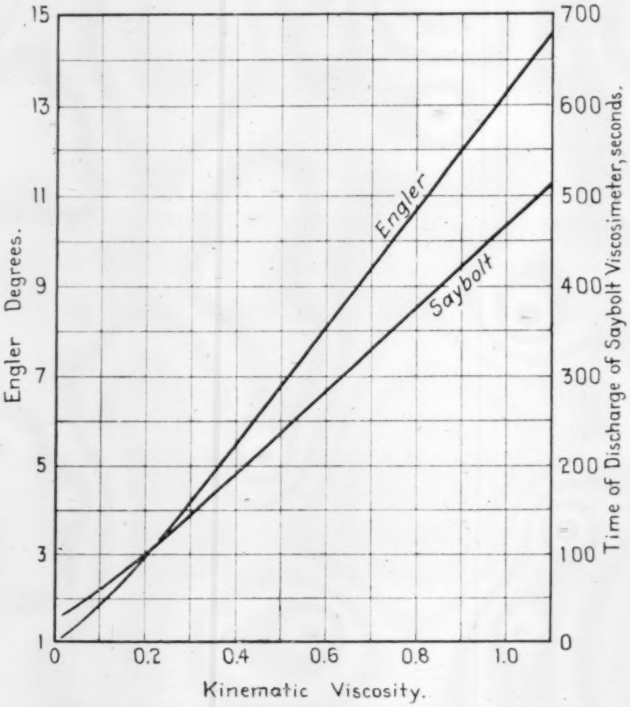

FIG. 5.—Plot of New Equations for the Saybolt Universal and Engler Viscosimeters.

Fig. 4 gives values of k from which the viscosity may be calculated by Eq. 10. The curves have not been extended to times of discharge less than 56 seconds, Engler, or 31 seconds, Saybolt, as Eqs. 8 and 9 do not hold good, and changes in the value of k are so rapid that accurate determinations of viscosity are impossible. For times of discharge over say 300 seconds, Saybolt, or 400 seconds, Engler, the kinematic viscosity is substantially proportional to the time of discharge.

The curves of Fig. 5 were plotted from Eqs. 8 and 9. Fig. 6 may be used for conversions between the different methods of expressing so-called viscosity, commonly in use in the United States and Germany.[1] Curves have been drawn for approximate estimates of the readings at the commonly used temperatures, the assumed rate of change of viscosity with change

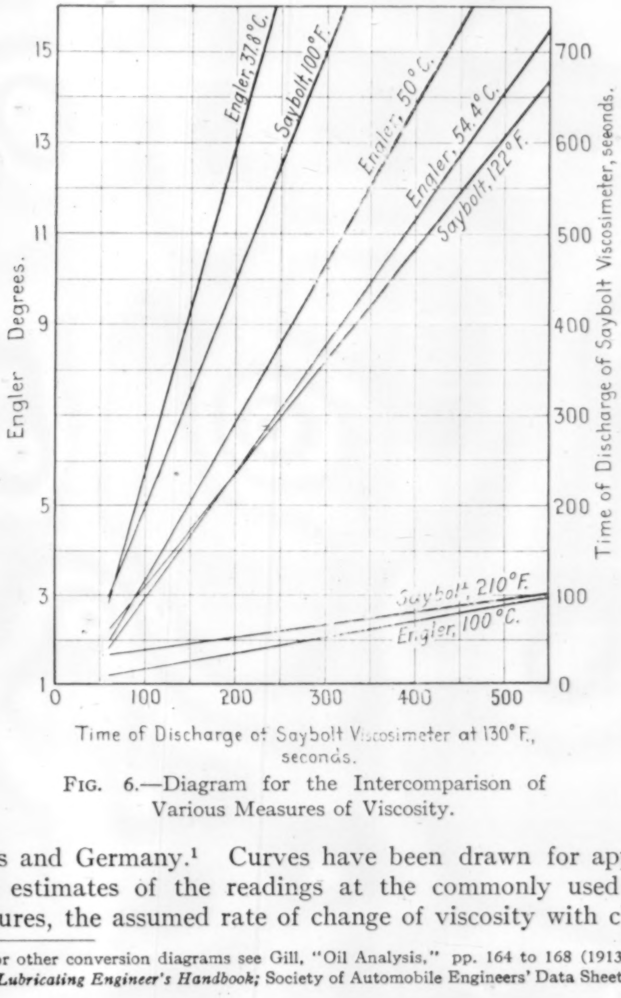

FIG. 6.—Diagram for the Intercomparison of Various Measures of Viscosity.

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[1] For other conversion diagrams see Gill, "Oil Analysis," pp. 164 to 168 (1913); J. R. Battle, *Lubricating Engineer's Handbook;* Society of Automobile Engineers' Data Sheets, Vol. 2, p. 18.

of temperature being that which may be expected, on the average, with oils having a paraffin base. Oils with an asphalt base may show a higher rate, and compounded oils a lower rate of change. The temperatures of 100°, 130°, and 210° F. were recommended for adoption as standard by a committee of the American Society for Testing Materials,[1] while 50° and 100° C. are temperatures commonly employed with the Engler viscosimeter.[2]

To illustrate the use of Fig. 6 the ordinate indicating the Saybolt time of discharge of 200 seconds may be chosen, and the points noted where it crosses successively the seven lines on the diagram. The readings show that an oil having this time of discharge at a temperature of 130° F. will have a time, Saybolt, of 55 seconds, at 210° F., which corresponds to a reading, in Engler degrees, of 1.7 at 100° C. For the purposes of this approximate estimate, the viscosity is practically the same at 100° C. (212° F.) as at 210° F. (98°.9 C.). The reading at 122° F. (50° C.) would be 235 seconds, Saybolt, or 6.8 Engler degrees, while at 54°.4 C. (130° F.) the reading would be 5.7 Engler degrees. Finally, at 100° F. (37°.8 C.) the readings would be 450 seconds, Saybolt, and 12.9 Engler degrees.

CONCLUSIONS.

The value of the constants in Eq. 1, for determining absolute viscosity by the capillary tube method, might be computed from the dimensions of the viscosimeter if an accurate formula were available for finding the average head on the tube, and if the effective length of tube could be determined.

This investigation has shown that the effective length is always slightly greater than the actual, but sufficient information is not available to calculate the effective length from the measured dimensions. The effective head could be obtained by subtracting 1.12 times the square of the average velocity, divided by the acceleration of gravity, from the total average head, if this could be correctly determined. Since, however, an approximate formula for average head must be used, a lower value than 1.12 must be employed.

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part I, p. 280 (1915).

[2] D. Holde, ***Zeitschrift***, Der Vere eutscher Ingenieure, Vol. 56, Part 2, p. 1461 (1912).

On account of these difficulties with short-tube viscosimeters in which the pressure is produced by a head of the liquid being tested, it is necessary to determine the constants of Eq. 1 by finding the times of discharge of two liquids of known viscosity. The viscosity is best determined in a long-tube viscosimeter, for which the constants may be calculated with sufficient accuracy from the dimensions of the instrument. Care must be taken, however, that the critical velocity is not exceeded at which Eq. 1 ceases to be valid. Neglect of this precaution has caused all equations, based on tests with water, for instruments having tubes of about 7 or 8 diameters in length, to be in error.

It remains to point out some desirable fields for further work.

1. Assuming that the short-tube voscosimeter will not be soon superseded for technical work, there is still a need for scientific purposes of a self-contained, portable and durable viscosimeter by which absolute viscosity may be measured. Long-tube viscosimeters, as at present constructed, are neither self-contained nor durable. The suspending wire of torsion viscosimeters is easily twisted too far so that a recalibration is required. These instruments have the advantage, however, over capillary tube instruments, in speed of operation, especially when a viscosity-temperature curve is to be obtained, but the conditions affecting their accuracy require further investigation.

2. It is very desirable that equations of the form of Eq. 1, with suitable tolerances in the constants and limits of applicability, should be adopted by the American Society for Testing Materials, and other interested associations, for the short-tube viscosimeters in common use, a prerequisite being the adoption of standard dimensions. The use of other short-tube instruments, not thus recognized, should be discontinued.

3. There is a great need of standard liquids, of moderately high viscosity, by means of which each user of a viscosimeter could test the accuracy of his instrument. The highest viscosity used should be such that it may be assumed without serious error that the ratio of kinematic viscosity to time of discharge is constant for all higher viscosities. But if the viscosity is too high, owing to the rapid change of viscosity with the temperature, the experimental error will be greater than that involved in the

above assumption. If, for example, it is assumed that for all practical purposes *k* of Eq. 10 may be assumed constant for all Saybolt times of discharge over 300 seconds, then from Eq. 8, the highest kinematic viscosity required will be 0.639. That is, if an oil were used having a density of 0.8, the absolute viscosity would be 0.511 poises.

DISCUSSION.

Mr. Flowers.

Mr. A. E. Flowers.—This paper is another important contribution looking to the statement of viscosity results in terms that imply real viscosity. The unfortunate fact is, and of course it has been brought out in this paper as in others, that most of our technical instruments, while giving satisfactory results to the trade, give satisfactory results so far as actual viscosity is concerned only for rather high viscosities. The curves shown in Fig. 5 give a very definite idea of how far one can extend the results towards the lower ranges with any approach toward a result which is proportional to the real viscosity. I consider it a very definite contribution towards the work on this subject that these curves should be presented here. You will note on the curves that the constants by which one could convert the readings to something proportional to viscosity are in themselves constants only for values of time on the Engler instrument of about 250 seconds, or on the Saybolt of about 150 seconds. Now both of those instruments are supposed to be standardized with water at 20° C., and the constant at the lower end of those curves is something like one-fifth to one-sixth of the value of the constant where it becomes a true constant. That means, moreover, that the viscosity of the standard material, water, instead of being something like 51 to 250 or 26 to 150 in the Engler and Saybolt instruments respectively,—that is, $\frac{1}{5}$,—is about $\frac{1}{30}$ of the viscosity at the point where the readings become true viscosity.

Mr. Herschel quotes from some preceding work in which numerical values are given for constants A and B, and states that in determining these constants he would limit his work to values of kinematic viscosity much higher than those previously used in determining them, and specifically throws out the use of water for a satisfactory material. If you will compare the equations in Table I of the paper with the constants which he obtains himself, it will be seen that the difference is not so very large. All of the equations stated there, except Mr. Her-

Mr. Flowers. schel's own, I believe, were taken with water as one of the materials. In one of the cases, for instance, comparing directly the constant *A* (which is the dominating constant when viscous flow may be said to control the flow rather than the condition when hydraulic resistance controls the flow as determined by this equation), it will be seen that the difference in the constant is not so very large, 263 for instance as against 260, a matter of 2 or 3 per cent. In view of the other errors that are present, that is not in itself a very large error.

EFFECT OF CONTROLLABLE VARIABLES ON THE TOUGHNESS TEST FOR ROCK.

By F. H. Jackson, Jr.

SUMMARY.

The object of this investigation was to study the effect of certain controllable variables on the accuracy of the toughness test for rock, with the view of using the data obtained in connection with the proposed revision of the standard test of the Society. The following possible reasons for discrepancies in results of tests are discussed:

1. Variations in the toughness of material from the same quarry, taken from time to time during the progress of quarrying operations.

2. Variations in the toughness of material taken from different portions of the same quarry at the same time.

3. Variations in methods of preparing test specimens, including, effect of:

(*a*) Direction of drilling test cylinder with reference to possible planes of weakness in the material;

(*b*) Slight variations in either the diameter or height of specimen;

(*c*) Variations in moisture content of specimen; and

(*d*) Poorly prepared bearing surface of specimen.

4. Variations in methods of testing.

The following conclusions have been drawn:

1. The quality of the product of a rock quarry may vary from time to time to such an extent, that it is advisable to test the material, as nearly as possible, at the time it is to be used.

2. Great care should be exercised in selecting samples for the toughness test in order to insure obtaining an average value for the toughness of the entire product.

3. All materials exhibiting any indication of foliation or bedding should be tested in two directions; one set perpendicular, and the other set parallel to the foliations.

4. A greater total variation than 1 mm. in either the diameter or height of the test specimen should not be allowed.

5. Variations in the moisture content of test specimens affect the results very little, although for purposes of uniformity it is recommended that all specimens be dried out prior to testing.

6. Very accurately prepared bearing surfaces are essential if reliable results are to be expected.

7. The effect of vibrations of the plunger and secondary blows produced by the rebound of the hammer during the progress of the test are apparently negligible.

EFFECT OF CONTROLLABLE VARIABLES ON THE TOUGHNESS TEST FOR ROCK.

By F. H. Jackson, Jr.

This paper presents the results of a study of the standard toughness test for rock, which has recently been made in the laboratory of the U. S. Office of Public Roads and Rural Engineering. The test was introduced into the United States about fifteen years ago by Mr. L. W. Page, who was at that time director of the road material laboratory of the Massachusetts Highway Commission. It was made a standard of this Society in 1908 and has been in very general use in highway testing laboratories throughout the country since that time. The general principles involved are given in the standard.[1]

A detailed description of the method employed in the government laboratory has likewise been published;[2] hence, a description of the test will not be given here. In general, it has been considered very satisfactory for the purpose for which it was designed, that is, to measure the relative resistance to impact of road building rock. Certain discrepancies have been noted recently, however, between tests made in the same laboratory on samples from different portions of a rock quarry, as well as between tests made by different laboratories on apparently identical material. These discrepancies would seem to indicate that the variations in the physical properties of rock from the same quarry may be much greater than has been heretofore considered probable, and also that the method of conducting the test is perhaps not sufficiently controlled under the present standard to insure check results in all cases. In order to obtain some quantitative preliminary data regarding the second point, a cooperative set of tests was recently made by five road-material testing laboratories. The toughness of the same apparently homogeneous sample of trap rock was obtained independently by each laboratory, four determinations of the toughness being made in every case but one. The results obtained are shown in Table I.

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[1] Standard Test for Toughness of Macadam Rock (Serial Designation: D 3-08), 1916 Book of A.S.T.M. Standards, p. 526.

[2] *Bulletin No. 347*, U. S. Department of Agriculture.

The large variations shown demonstrates the need of an investigation of the possible cause of the discrepancies with the view of obtaining data to be used as the basis for the formulation of a revised standard.

An analysis of the methods now used in making the toughness test shows that discrepancies in results may in general be traced to either (1) the fact that the range in physical properties of the quarry product is so great that it is impossible to obtain an average value for the toughness of the material, with the small size and number of test specimens used in a given determination, or (2) to the fact that certain variables may be introduced during the preparation or testing of the specimen which might affect the result and which are not controlled by the present standard. These considerations are of course entirely distinct from any question of the value of a test for impact, as an indication of the suitability of rock for road-building purposes.

TABLE I.—RESULTS OF COOPERATIVE TOUGHNESS TESTS.

Name of Laboratory.	Toughness; Height of Fall Causing Failure, cm.				
	Test No.				Average.
	1	2	3	4	
New York State	19	20	16	16	18
Pennsylvania State	27	29	28	33	29
Ohio State	18	19	..	..	19
Purdue University	29	34	31	25	30
Office of Public Roads	14	17	16	15	16

A comprehensive consideration of the variables affecting the accuracy of the test must include a study of the following:

VARIATIONS IN TOUGHNESS OF ROCK FROM SAME QUARRY.

A determination of the probable extent of this variation is very important for obvious reasons. For instance, there is a general impression current among those interested commercially in the production of stone for road work, that the results of a single test, made perhaps several years before, is sufficient from which to judge the quality of a rock quarry. Likewise, in sampling rock for test, it is usually thought sufficient to take a single sample without any particular care being exercised to insure having it represent the only kind of material which it is proposed to use. The results of a few tests of samples from a number of well-known quarries, which have been in operation for some years, are given in Table II, and show the wide range in toughness which it is possible to obtain on material from the same quarry. Although all of these tests were made in a single laboratory, using the same testing machine and tested in most cases by the same operators, and were therefore presumably made under fairly uniform conditions, it is quite possible that some of the variations indicated are due to a failure to appreciate, and therefore control, variables introduced during either the preparation or testing of the specimens. In order to throw some light on this point, the actual variations in the quality of the product of several quarries were studied under as nearly as possible identical conditions.

TABLE II.—VARIATION IN TOUGHNESS OF ROCK FROM SAME QUARRY.

SAMPLES TAKEN AT DIFFERENT TIMES.

Location of Quarry.	Kind of Rock.	Toughness; Height of Fall Causing Failure, cm.					
		Test No.					
		1	2	3	4	5	6
New York	Trap	27	27	17	33	21	23
New Jersey	Trap	27	33	30	..	..	..
Pennsylvania	Trap	12	26	17	13	..	..
Ohio	Limestone	18	9	6	7	..	..
Ohio	Limestone	16	11	10	..	..	..
New York	Limestone	9	7	13	13	..	..
Maryland	Schist	20	19	34	30	44	..
North Carolina	Granite	13	14	9	10	10	11
New Hampshire	Granite	7	9	8	8	..	..

Table III gives results showing the possible variation in toughness of several typical rock quarries when the samples are taken at the same time from different portions of the deposit. These results are of principal interest in showing the wide range in toughness possible to obtain on samples taken from different portions of the same quarry, even when the material quarried is considered to be very uniform in quality, which is the case in both the trap-rock and granite quarries examined. In the case of two of the trap-rock quarries, there was also noted a considerable variation in the toughness of the individual specimens composing a sample. The individual breaks are given in Table IV in connection with the study of the effect of direction of drilling on toughness.

TABLE III.—VARIATION IN TOUGHNESS OF ROCK FROM SAME QUARRY.

SAMPLES TAKEN AT SAME TIME.

Location of Quarry.	Kind of Rock.	Average Toughness; Height of Fall Causing Failure, cm.							
		Sample No.						General Average.	Maximum Variation.
		1	2	3	4	5	6		
New York............	Trap..........	20[a]	20[b]	15[b]	22[c]	..	..	19	7
New Jersey..........	Trap..........	29[b]	33[d]	35[b]	..	..	..	32	6
Maryland............	Trap..........	20[e]	10[e]	20[e]	..	..	..	20	1
Maine...............	Granite.......	18[f]	16[f]	16[f]	..	..	..	17	2
Massachusetts........	Granite.......	9[f]	10[f]	10[f]	..	..	..	10	1
Maryland............	Granite.......	10[g]	10[h]	10[g]	..	..	..	10	0
Ohio................	Limestone.....	12	7	8	14	16	12	12	9
Ohio................	Limestone.....	6	8	7	6	6	5	6	3
Ohio................	Limestone.....	12	8	6	9	11	10	9	6

[a] Average of 12 specimens.
[b] Average of 10 specimens.
[c] Average of 11 specimens.
[d] Average of 8 specimens.
[e] Average of 5 specimens.
[f] Average of 2 specimens.
[g] Average of 3 specimens.
[h] Average of 4 specimens.

It was noted at the time the trap-rock specimens were broken, that the ones showing the most irregular fracture at failure were in general the ones showing the lowest results, even when there was absolutely no visible line of weakness before failure. The appearance of these peculiar fractures, which every experienced operator has noticed, would seem to indicate that there may be present in some rocks, a great number of minute planes of weakness, running in all directions and formed possibly at the same time and in much the same way as the large joint planes with which the quarryman is familiar. The variable toughness shown by different specimens cut from the same piece might also be caused by the formation of incipient fractures in the mass due to blasting. Conditions such as these are emphasized by the comparatively small-size test specimens and might form an argument for the adoption of a larger size. When we consider, however, the increased cost of and the time necessary to prepare larger specimens, it would appear advisable to use the present size, and to obtain an average value for the toughness by making a larger number of tests than is at present customary.

In the case of the granites examined, much greater uniformity was observed, not only between different samples from the same quarry, but also between individual specimens taken from the same sample. The limestones tested represent probably the extreme of non-homogeneity apt to be found in ordinary road-building rock and are given for this reason. Each of these materials was extremely non-homogeneous in appearance, individual pieces running from fairly tough, fine-grained rock to portions showing a high percentage of argillaceous material, which runs of course much lower in toughness. It is obvious that the only way to obtain average values for the toughness of material from deposits such as these, is to exercise extreme care in selecting samples to insure that all variations in quality are represented. This would, of course, entail more work than the mere picking of a single sample, which is common practice.

An interesting feature in connection with the limestone tests is the fact that considerable variation was also noted in even the standard abrasion test, which utilizes a very much larger sample. In the first case two abrasion tests gave French coefficients of wear of 13.3 and 10.5; in the second case, 12.1 and 10.3; and in the third case, 18.2 and 10.5.

The results given in Table III are presented for the purpose of showing quantitatively the possible bearing on the test result of lack of care in sampling. Unless there is reasonable assurance that the material tested is representative, no amount of care in the preparation and testing of the individual samples can be expected to secure reliable results.

VARIABLES INTRODUCED DURING THE PREPARATION OF THE TEST SPECIMEN.

Variables occurring during preparation of the test specimen may include (1) effect of direction of drilling specimens with reference to any possible plane of structural weakness, such as rift, bedding plane, or plane of foliation; (2) effect of size of test specimen, either in diameter or height; (3) effect of moisture content; and (4) effect of lack of care in preparation of specimen, such as failure to have perfectly plane bearing surfaces, or ends absolutely perpendicular to the axis of the cylinder.

With reference to the first point it has been the custom in most laboratories to drill specimens from a rock sample at random, except in cases where well-developed, visible planes of foliation appear, in which case specimens are usually taken perpendicular to the foliation. The question has been recently raised, however, whether other materials might not possibly have planes of weakness, which would cause specimens drilled in one direction to fail sooner than those drilled in the other direction. Thus, granites drilled along the rift, and sedimentary rocks, such as limestone and sandstone drilled along the bedding plane, might show lower toughness values than when drilled in other directions. The results of the author's study along this line are given in Table IV.

The eight granites examined represented material from some of the best-known granite quarries in the United States. In each case, results recorded under sample set A were obtained on material drilled perpendicular to set B. The samples were full-size paving blocks, and the individual specimens were drilled along the two short dimensions; sample set B being drilled in the direction of laying in the street. It is seen at once that there is very little difference in the average toughness of sets A and B. In fact the difference is so slight as to be practically negligible.

When we come to the foliated types, however, as represented by the gneisses and schists, we find as would be expected, a

TABLE IV.—EFFECT OF DIRECTION OF DRILLING SPECIMEN ON TOUGHNESS.

Location of Quarry.	Kind of Rock.	Sample Set.	Toughness; Height of Fall Causing Failure, cm.						
			Specimen No.						Average.
			1	2	3	4	5	6	
Pennsylvania.........	Granite.....	A	9	8	9	11	9	8	9
		B	9	11	10	10	11	10	10
Pennsylvania.........	Granite.....	A	8	6	6	7	7	6	7
		B	7	5	6	7	6	8	7
Maine...............	Granite.....	A	7	9	8	8	9	8	8
		B	10	9	8	9	7	..	9
Virginia.............	Granite.....	A	8	7	5	4	6	6	6
		B	6	6	7	6	6	6	6
Massachusetts........	Granite.....	A	10	7	9	8	10	8	9
		B	8	8	10	9	11	11	10
Maine...............	Granite.....	A	10	11	10	12	11	12	11
		B	10	10	12	10	10	11	11
North Carolina.......	Granite.....	A	8	9	7	6	6	7	7
		B	5	5	7	6	6	8	6
Maine...............	Granite.....	A	7	7	7	6	6	..	7
		B	9	8	9	7	6	7	8
New York...........	Gneiss......	A	13	12	12	12	16	11	13
		B	6	7	6	6	6	7	6
Maryland............	Schist.......	A	32	30	30	..	..	..	30
		B	16	12	9	..	..	..	12
New York...........	Limestone..	A	13	14	13	..	..	..	13
		B	12	10	12	..	..	..	12
Indiana..............	Limestone..	A	11	12	10	10	..	..	11
		B	9	10	12	10	..	..	10
West Virginia........	Sandstone...	A	11	13	10	13	9	13	12
		B	9	8	8	9	7	6	8
	Sandstone...	A	11	12	11	9	11	11	11
		B	11	10	11	11	10	10	11
Virginia.............	Sandstone...	A	10	10	10	9	10	..	10
		B	7	7	7	7	..	..	7
New York...........	Trap........	A	21	17	21	17	19	19	19
		B	21	19	25	23	19	20	21
New York...........	Trap........	A	22	19	20	20	22	..	21
		B	19	19	26	16	20	..	20
New Jersey..........	Trap........	A	27	27	28	30	38	..	30
		B	24	29	25	30	27	..	27
New Jersey..........	Trap........	A	32	33	32	39	33	35	34
		B	34	33	36	38	..	..	35

A...... Specimen drilled perpendicular to the direction of laying in the street.
B...... Specimen drilled in the direction of laying in the street.

large difference in toughness, the specimens drilled perpendicular to the foliations being in one case twice, and in the other case almost three times as tough as those drilled parallel to them.

In regard to the sedimentary rocks, only slight variations are noted, with two exceptions, namely, the first and third sandstones tabulated. Both of these materials showed thin, well-developed planes of bedding. In the case of the other sandstone, as well as both of the limestones, the stratification was not nearly so well marked. These results indicate that in certain cases at least, the bedding plane of sedimentary rocks must be considered in preparing specimens for test. In the case of both the sedimentary and foliated types, the specimens of set A were drilled perpendicular, and the specimens of set B parallel, to the plane of weakness.

The results on four samples of trap rock are also given, and indicate very little difference in toughness due to the direction of drilling. The two sets of specimens in these cases were drilled perpendicular to each other, without any reference to the natural bed of the stone, since true bedding, so-called, does not exist in the massive trap-rock formations. The individual results of breaks given in Table IV indicate the possible variation in toughness of different specimens taken from the same piece, and tested under identical conditions. This latter point has been discussed under the head of variations in quarry samples.

The reason for the large number of tests of granite compared to the other types is because of the possible desirability of using this test in connection with the use of granite in stone-block construction. As may be seen, there is not only considerable range in toughness of the different granites tested, but the individual specimens in every case lie very close to the average, which indicates that it should not be a difficult matter to obtain check results.

Variation in both the diameter and height of a test specimen should at first sight be easily controlled during its preparation. Experience has shown, however, that it is practically impossible to manufacture diamond drills which will drill cores of constant diameter, the wearing away of the diamonds resulting in producing cylinders of gradually increasing diameter.

This condition naturally results in tests being made on specimens covering a considerable range in diameter with consequent possible variation in result. In Table V are given the results of a study of the effect of this variable, made with the view of determining how great a permissible variation in diameter should be allowed.

Five materials showing different degrees of toughness were examined and in all cases but one, marked variations were observed. For instance, in the case of the trap-rock an increase of 3 mm. in diameter resulted in an increase in toughness of 36 per cent. In the case of the sandstone, a difference of 2 mm. in diameter resulted in a 50-per-cent increase in toughness. These variations would go far towards explaining the discrepancies noted in Table I, even assuming all other variables to be of negligible importance, and certainly shows the necessity of maintaining the diameter of the specimen at as near 25 mm. as possible. The experience of the author covering a period of several years in the government laboratory would indicate that

TABLE V.—EFFECT OF DIAMETER OF SPECIMEN ON TOUGHNESS.

Location of Quarry.	Kind of Rock.	Diameter, mm.	Toughness; Height of Fall Causing Failure, cm.						
			Specimen No.						Average.
			1	2	3	4	5	6	
Ohio...............	Limestone...	18.0	2	3	2	3	3	3	3
		23.5	2	3	3	3	..	..	3
		24.5	3	3	3	3	..	..	3
		25.0	3	2	3	3	..	..	3
	Sandstone...	18.5	8	6	7	7	6	7	7
		23.0	9	9	8	8	9	6	8
		24.0	11	10	11	11	10	10	11
		25.0	13	13	11	11	12	13	12
Maryland............	Granite.....	19.0	5	7	6	6	6	7	6
		24.0	11	10	10	11	10	10	10
		25.0	12	12	11	12	13	15	13
Maryland............	Trap.......	19.0	16	16	15	14	15	..	15
		23.0	19	19	19	20	..	..	19
		24.0	20	18	21	21	..	..	20
		25.0	23	22	22	22	24	21	22
		26.0	25	27	27	26	..	..	26
	Granite.....	23.0	8	8	7	6	8	..	8
		24.0	10	10	9	9	10	..	10
		25.0	11	11	11	10	..	..	11

a permissible variation of less than 1 mm. would not be practical from the standpoint of drill manufacture. No greater latitude should be allowed, however, under any circumstances.

The matter of height of specimen is, of course, not difficult to control, since it is a comparatively simple matter to prepare cylinders almost exactly 25 mm. long. A few comparative tests were made, however, which indicated that a range of as much as 1 mm. might be allowed without affecting the result.

TABLE VI.—EFFECT OF MOISTURE CONTENT OF SPECIMEN ON TOUGHNESS.

Kind of Rock.	Condition of Specimen.	Toughness: Height of Fall Causing Failure, cm.				
		Specimen No.				Average.
		1	2	3	4	
Sandstone	Dry	11	10	11	11	11
	Saturated	11	9	9	10	10
Granite	Dry	12	12	11	13	12
	Saturated	8	12	10	10	10
Trap	Dry	19	19	19	19	19
	Saturated	18	19	18	20	19
Dolomite	Dry	10	12	14	..	12
	Saturated	10	9	10	..	10
Limestone	Dry	9	9	..	..	9
	Saturated	9	9	..	..	9
Quartzite	Dry	21	17	19	..	19
	Saturated	18	18	16	..	18

The question of the moisture content of specimens at time of testing and its possible effect on toughness was also considered. The results of the tests are given in Table VI. As may be seen, a slightly lower toughness appears in some cases to result from saturating the specimens prior to testing. The differences are so slight, however, that they may be accidental. In any event they are not great enough to be of practical importance in routine testing.

One other point in connection with the preparation of the specimen remains to be mentioned, and this is the effect of lack of care in the preparation of the faces of the cylinder, resulting in uneven bearing and eccentric loading. The effects of either of these conditions is so obvious, that only enough data will be presented to give some idea quantitatively of the enormous error introduced by careless preparation of the test specimen. For instance, a sample of granite with a normal toughness of 12, showed an average toughness of only 7 when tested under only slightly imperfect bearing conditions, and an average toughness of 8 when subjected to eccentric loading—that is, when the axis of the specimen was not exactly vertical, thereby throwing the tangent point of the plunger slightly to one side. It seems almost unnecessary to emphasize these points, but experience has shown that operators, unless carefully supervised, are apt to slight the final preparation of a test specimen, under the impression that it makes very little difference in the result.

VARIABLES INTRODUCED DURING THE TEST.

Variables occurring during testing should include (1) such variations in the design of the testing machine as might affect the test, such as different devices for holding the specimen in place; (2) the degree of freedom with which the plunger is allowed to slide vertically; and (3) the effect of secondary blows produced by the rebound of the hammer. It is regretted that lack of time has prevented any study of the first of these points, because it is quite possible that large discrepancies might result from the use of different types of grips, on account of variations in the amount of lateral pressure brought to bear on the specimen during the test. In all of the tests made in connection with this investigation, the form originally devised by Mr. L. W. Page,[1] and which has proved very satisfactory, was used.

In regard to the second point, the present standard test specifies that the plunger shall be held in place over the specimen by means of springs. As a matter of fact, springs were never used except at first, the plunger resting upon the test specimen and being capable of free vertical movement in a sleeve. This results in considerable vibration in the plunger after each blow of the hammer, which is especially noticeable when tough materials are being tested. A few tests were made to determine the effect of this vibration on the toughness, the idea being that the amount of vibration with consequent local damage to the specimen might vary with different testing machines. Samples of the traps and granites used in this study were tested by holding the plunger rigidly in position over the test specimen until failure occurred, thereby eliminating all vibration. In no case was the difference in toughness, as compared to the normal result, great enough to be of practical moment, indicating thereby that the vibration of the plunger does not actually weaken the resistance to impact of the specimen. This point is of interest because the test has been criticised in this connection.

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[1] *Bulletin No. 347*, U. S. Dept. of Agriculture.

The only other point remaining to be discussed is the effect on the test result of the secondary blow produced by the rebound of the hammer. This secondary blow varies in intensity with the toughness of the material, reaching as high as 25 or 30 per cent of the total fall of the hammer with very tough materials. To determine the effect of this action on fairly tough materials, specimens taken from a sample of trap rock showing a normal toughness of 29, were subjected to repeated impact from a height of 20 cm. until failure occurred. In one case 27, in another case 36, and in still another case 41 blows of the hammer from this height were required to break the specimen, indicating clearly that the secondary blow of the hammer can have no practical effect in weakening the specimen. These results are interesting from a practical standpoint in indicating that no changes in the actual conduct of the test are necessary to produce accurate results. In fact, the actual testing of the specimen is the simplest part of all, the real difficulty in obtaining reliable results lying, as has been stated, in the sampling of the material and the preparation of the specimen.

CONCLUSIONS.

The following points in connection with the work described may be noted:

1. In judging the quality of a rock quarry from the results of a single test, the fact that large variations in toughness may occur from time to time should be borne in mind, and an effort

made to secure a test of the material at approximately the same time that the material is to be used.

2. Great care should always be exercised in selecting samples for the toughness test, because the product of a rock quarry of even apparently uniform quality is apt to vary, throughout different portions of the face.

3. In preparing samples for the toughness test, the following points should be kept in mind and the operator governed accordingly:

(*a*) In every case where planes of foliation as in all gneisses and schists, or planes of bedding as in many sandstones and some limestones, are visible, specimens should be drilled as nearly at right angles to the plane of weakness as possible. Another set of specimens should also be prepared from samples drilled parallel to the plane of weakness;

(***b***) No core drill should be used which drills specimens with a greater total variation than 1 mm.;

(*c*) Variations in the height of specimens may be as great as 1 mm. without practically affecting the result;

(*d*) Variations in the moisture content of specimens apparently do not practically affect the result. For the sake of uniformity, however, all specimens should be dried in an oven prior to testing; and

(*e*) The greatest possible care should be exercised in the preparation of the specimen, so as to insure perfect bearing surfaces absolutely perpendicular to the axis of the cylinder.

4. Very little injury is done to a specimen during the progress of the test as a result of vibrations of the plunger.

5. The total energy of the blow required to cause failure is not influenced by secondary blows of the hammer, produced by its rebound after striking the plunger.

DISCUSSION.

Mr. Page. MR. L. W. PAGE.—While this toughness test of rock was developed by myself, I am free to admit it is rather an arbitrary one, although it runs more uniformly than any other test of rock that I know of. It really determines the elastic limit of the rock; that is, a piece 2 in. high will break with the same blow as one 1 in. high. Mr. Jackson refers to the variation of rock in the same quarry. The physical qualities of rock in the same quarry vary enormously. We speak of trap rock as being uniform but I should say that trap rock varies more than many other types. There are trap dikes where the molten rock was forced up through the country rock under enormous pressure from very far below the existing surface at that time, and it came in contact at high temperature with the country rock. The rock at the walls of such a dike cooled very quickly after coming in contact with the country rock, and is as fine grained as limestone or slate. Toward the center of the dike, the rock becomes very coarse grained on account of the slower cooling. In fact, I think of all the rocks that I know of, trap rock probably varies in physical qualities more than any other, although its appearance is more uniform.

The Government has for some twenty years been making physical tests of rock free of charge for any one who will send in a sample of rock. Now the responsibility of obtaining representative test results must of necessity rest with the person sending in the samples of rock to be tested. If a quarry of any size is to be thoroughly tested, I should say that at least 10 samples should be selected, taking the extreme of conditions in the quarry; but as it is done for nothing, the average person will send the Government a single sample of rock, regardless of how close it was to the blast, and if from a dike, how close it was to the side walls of the quarry. Naturally, therefore, all sorts of results are obtained according to the particular sample submitted. My recommendation for the past twenty years has been that to obtain average results a large number of tests must be made; one test does not mean very much.

Mr. Macgregor. MR. J. S. MACGREGOR.—I should like to ask the author of this paper whether he investigated the lack of parallelism between the ends of the specimens. Any condition such as this, which would cause the impact to be not perfectly normal to the end of the specimen, would have a marked effect on the results which would be obtained.

Mr. Reinecke. MR. L. REINECKE.—There is just one point brought out both by Mr. Jackson and Mr. Page that I should like to emphasize. It is evident that the value of the toughness test will depend on whether the toughness of the sample represents the toughness of the product of the quarry. At present[1] the toughness test is made on one block of 6 in. in greatest dimension, taken out of a quarry. In no more than 5 per cent of the quarries I have examined will a face of 30 ft. for instance, be uniform in toughness across the face within a value, say, of 2; in the other 95 per cent, the toughness values within vertical distances of 10 ft. might vary from 2 to 7 or 8. I am guessing at the variation to some extent, but I know that the strength and character of the rock vary enormously in 95 per cent of the quarries. I therefore believe that this test will not be truly useful until it is definitely stated in the specifications, and insisted on, that the toughness test be made from cores drilled from at least three blocks taken in different parts of the same vertical face of a quarry; and in some cases, when the variation is very great, that more blocks be used. I think that Mr. Jackson's paper is a very valuable one and that the refinements that he and Mr. Page have suggested will be of much assistance in obtaining accurate results in the laboratory. The toughness test is an important one, but I really think that tests should be made on more blocks taken from different parts of a quarry, in order to make it of real value.

Mr. Page. MR. PAGE.—I quite agree with what Mr. Reinecke has said. Moreover, it will be found that the matter of variation is not confined to the toughness test. All the tests will vary because the product varies greatly. A sample of rock taken from near a blast is thoroughly shattered, and will not show the same toughness as one taken remote from a blast; and the

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[1] Standard Test for Toughness of Macadam Rock, 1916 Book of A.S.T.M. Standards, p. 526.—ED.

Mr. Page. same is true of the resistance to wear and the hardness test. Of all the tests on road material I consider the hardness test of least value: it varies more than any of them; it has a smaller scientific basis than any other test, and shows less relation to the conditions of service.

Mr. Jackson. MR. F. H. JACKSON, JR.—In answer to Mr. Macgregor, very large variations in results may be caused by the faces of the test specimen not being absolutely parallel. In fact, as has been noted in the paper, errors as great as 100 per cent may be introduced by reason of very slight variations in this respect.

RAPID SEMI-AUTOGRAPHIC TESTS FOR DETERMINING THE PROPORTIONAL LIMIT.

By H. F. Moore.

SUMMARY.

A semi-autographic apparatus for obtaining in tension tests stress-strain diagrams within the proportional limit is described. One form of the apparatus requires two men to make a test, and another form requires only one man. In both forms an observer turns a record drum a definite distance each time the pointer of the extensometer passes a division on the extensometer dial.

Records of speed obtained with this apparatus are given, and sample diagrams shown.

RAPID SEMI-AUTOGRAPHIC TESTS FOR DETERMINING THE PROPORTIONAL LIMIT.

By H. F. Moore.

At meetings of this Society and on other occasions the attention of materials testing engineers has been called to the desirability of an apparatus for making rapid, yet accurate, determinations of the proportional limit of materials. Various ingenious autographic testing rigs have been proposed, but for the most part, such rigs have not been capable of great speed in use, and have been delicate and easily thrown out of adjustment.

During the past year the writer has built and used an autographic testing rig in the Materials Testing Laboratory of the University of Illinois, and a description of the apparatus and records of its performance are submitted in this paper.

It should be stated at the outset that the principle involved is by no means new. An autographic apparatus involving the principle used is described by Unwin,[1] and Lynch[2] described before this Society a method of locating the proportional limit which is somewhat like the method followed. The writer remembers a similar device used in 1896 by Prof. Albert Kingsbury on a torsion testing machine in the laboratories of the New Hampshire State College.

As applied to tension testing, the apparatus is shown in Fig. 1. Attached to the driving gear of the poise of a testing machine is a screw which moves the pencil *P* vertically through a distance proportional to the travel of the poise. The pencil point draws a record on a paper wrapped around the drum *D*, and the drum is rotated by means of a cord *C* which is attached to a sheave mounted on the same axle with the toothed wheel *W* which is fitted with a ratchet *R*. If the test is to be made without stopping the testing machine, two men are required to conduct the test. One man operates the poise of the weighing beam, keeping the beam balanced as the test proceeds and the other man watches the dial of an extensometer *E* which is attached to the specimen. This extensometer may be of any type which is self-indicating, that is, one in which no handling of the instrument is necessary in order to obtain a reading. As the pointer of the extensometer moves past each successive division mark on the dial the operator watching the extensometer moves the toothed wheel *W* one notch, turning the recording drum a short distance. This procedure is kept up until the yield point is reached, or until there is danger of rupture; the extensometer is then removed and the man at the beam keeps the beam in balance until the ultimate strength is reached. The testing machine is not stopped during the test, and is usually speeded up after the yield point is reached.

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[1] Unwin, "The Testing of the Materials of Construction," p. 246.

[2] T. D. Lynch, "Elastic Limit," *Proceedings*, Am. Soc. Test. Mats., Vol. XV, Part II, p. 415 (1915).

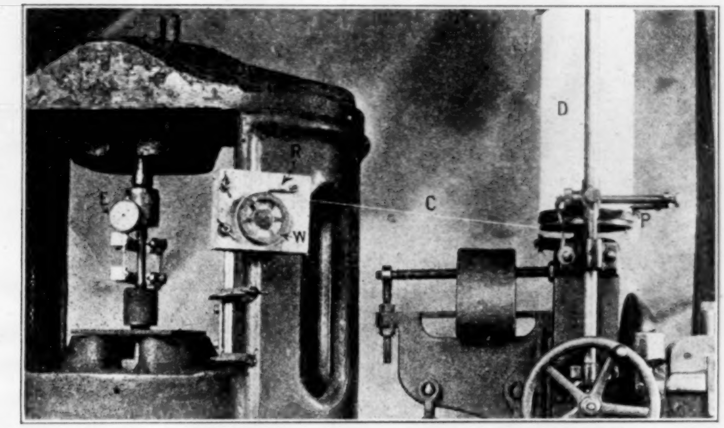

FIG. 1.—Semi-Autographic Testing Apparatus for Determining Proportional Limit.

A typical test record is shown in Fig. 2. This record consists of a number of steps decreasing in height as the proportional limit is passed. A curve connecting the edges of the steps gives the stress-strain diagram up to the yield point. In Fig. 2, solid lines are those actually recorded, and broken lines are construction lines used to locate the proportional limit *P*. The yield point is located at *Y* either by the "drop" of the beam or the sudden increase in extensometer readings (the man at the extensometer stopped turning the drum as soon as the yield point was reached), and the ultimate strength, at *U*, is given by the extreme height of the diagram.

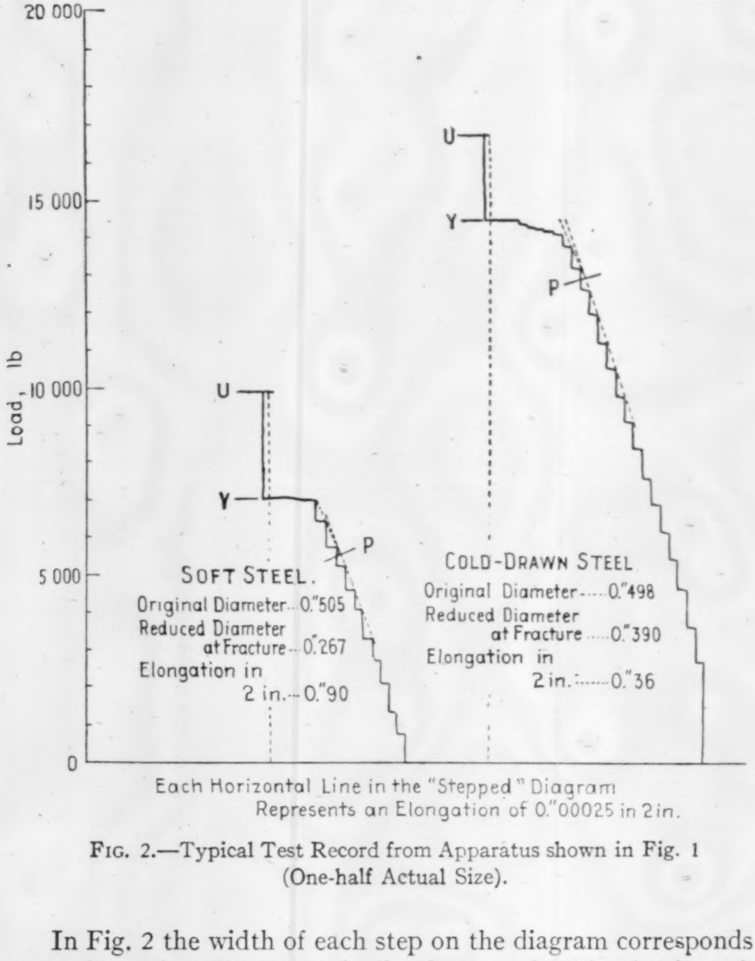

FIG. 2.—Typical Test Record from Apparatus shown in Fig. 1 (One-half Actual Size).

In Fig. 2 the width of each step on the diagram corresponds to an elongation of 0.00025 in. in the gage length of 2 in. A deviation of 0.0001 in. from Hooke's law could readily be detected in Fig. 2. The diagram traced by the autographic apparatus is double the size of Fig. 2.

As described above, the apparatus requires the services of two men during a test. An apparatus which can be handled by one man, and which, though slightly less accurate than the apparatus shown in Fig. 1, gives good results is shown in Fig. 3. The spring of a steam-engine indicator is used to keep the beam of the testing machine in balance, the poise remaining stationary during the test. Bearing on the beam at *T* is one end of a pointed rod, the upper end of which bears against the piston of a steam-engine indicator, whence the pressure is transmitted to the indicator spring. The pressure against the indicator spring, and consequently the vertical motion of the indicator pencil *P*, is proportional to the load applied to the specimen. The rotation of the indicator drum *D* is controlled by a cord *C* attached to a toothed wheel *W*, the arrangement being similar in every respect to the device for rotating the drum shown in Fig. 1. In using the apparatus shown in Fig. 3 the operator watches the dial of the extensometer *E*, and as the pointer passes each successive division on the dial he turns the toothed wheel one notch. Fig. 4 shows a typical diagram obtained with

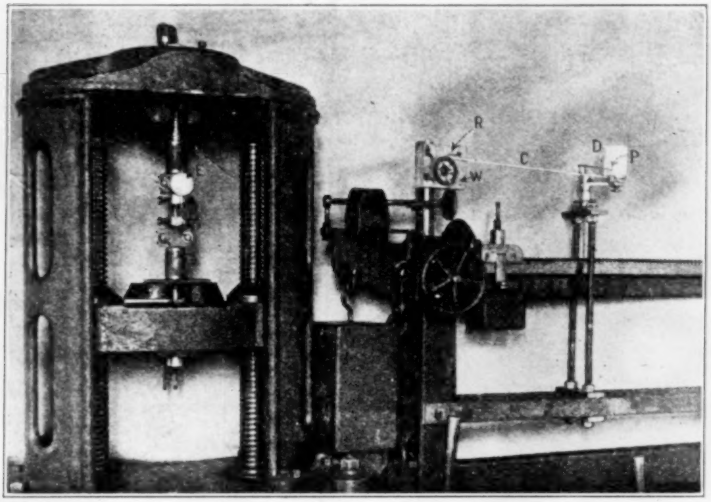

FIG. 3.—Semi-Autographic Testing Apparatus for Determining Proportional Limit.

this apparatus; solid lines show the diagram actually obtained, and broken lines are construction lines used in locating the proportional limit *P*. The yield point is located at *Y*, and the ultimate strength at *U*. The testing machine is kept running continuously during the test. In Fig. 4 the width of each step on the diagram corresponds to an elongation of 0.0005 in. in 2 in. The diagram traced by the autographic apparatus shown in Fig. 3 is half the size of Fig. 4.

The following speed of operation was attained by the writer and an assistant using the rig shown in Fig. 1 (two-man method) on a test of six 2-in. threaded-end specimens of iron and steel. The speed of head of testing machine was 0.1 in. per minute up to the yield point and 3 in. per minute beyond the yield point.

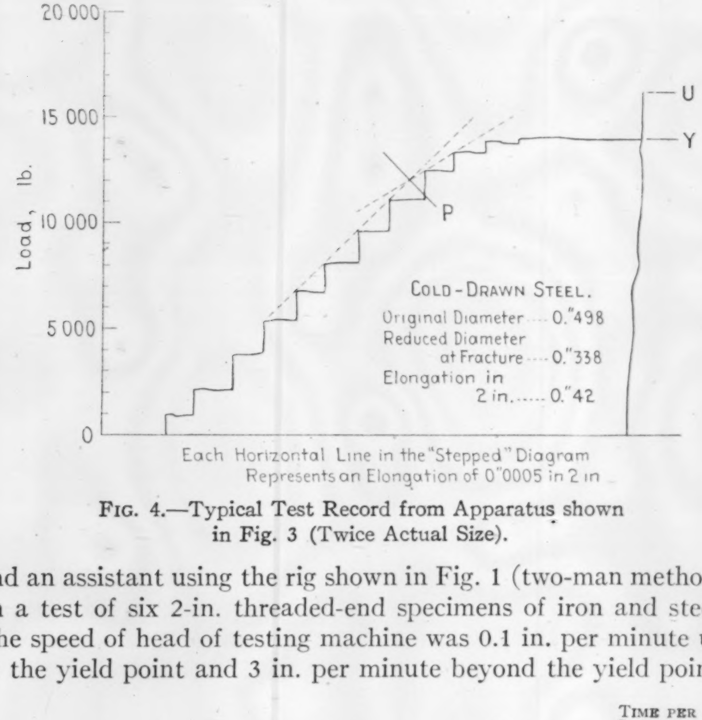

FIG. 4.—Typical Test Record from Apparatus shown in Fig. 3 (Twice Actual Size).

	TIME PER SPECIMEN.
Measuring, marking, testing, measuring elongation and reduced diameter	3 min. 10 sec.
Locating (on diagram) proportional limit, yield point, and ultimate strength, computing stresses and filling in report blank	1 min. 22 sec.
Total	4 min. 32 sec.

Using the rig shown in Fig. 3 (one-man method) the following speed of operation was attained for a test of ten 2-in., shouldered specimens of steel. The speed of the testing machine was 0.05 in. per minute up to the yield point, and 1.63 in. per minute beyond the yield point.

	TIME PER SPECIMEN.
Measuring and marking	0 min. 41 sec.
Testing, measuring elongation and reduced diameter	3 min. 51 sec.
Locating (on diagram) proportional limit, yield point, ultimate strength, computing stresses and filling in report blank	1 min. 52 sec.
Total	6 min. 24 sec.

Both of the arrangements of apparatus described above are rugged and simple. The extensometer used can be of any desired degree of sensitiveness, and the delicate parts of the instrument are not touched during the test. The proportional limit is determined from a diagram covering a series of readings, not from a single reading, and the smoothness of the curve outlined by the corners of the "steps" of the diagram gives an indication of the care and accuracy with which the test was made.

DISCUSSION.

Mr. Lasier. MR. E. L. LASIER (*by letter*).—The writer desires to comment not so much on Mr. Moore's paper, as on the allied subject of the obtaining of stress-strain curves in the testing of metals.

There certainly is need for the development of a quick and practicable, but fairly accurate means of determining the proportional limit. Current practice in mill testing laboratories is well known; results of routine tests may give a yield point by the usual drop-of-the-beam method, which may or may not be within 5 or even 10 per cent of the proportional limit, because of speed of testing, misleading drops of the beam due to slippage of the test specimen in the grips, and other causes familiar to laboratorians. It must be borne in mind too that usually the yield point obtained is in excess of the proportional limit, that is to say, on the unsafe rather than the safe side as far as design is concerned. In this connection, it is significant that Committee E-1 of this Society on Methods of Testing is recommending a reduction in the allowable pulling speeds at which testing should operate and is calling attention to the influence of the speed of testing upon tensile properties. It is also significant that in another paper[1] presented before this meeting, attention is invited to the need of testing the materials therein considered by methods other than those usually employed in the routine testing of metals.

Aside from the question of the determination of the proportional limit in lieu of the yield point, there is the general question of the value of stress-strain curves autographically plotted up to, at least, the maximum load which the specimen under test will sustain.

The May lecture to the Institute of Metals was given this year by Prof. W. E. Dally, F. R. S., upon the subject of Autographic Load-Extension Diagrams.[2] Professor Dally showed

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[1] Flinn and Jonson, "Inspection of Brass and Bronze," p. 212.—ED.

[2] *Engineering* (London), May 18, 1917.

Mr. Lasier.

some interesting curves for various metals, which are worthy of brief mention here. Curves for practically all of the non-ferrous metals which he investigated were continuous, with no break at the junction of the elastic and the plastic portions of the complete diagrams. Copper was treated in various ways without materially affecting the general shape of the curve recorded. On the other hand, the history of the brasses tested apparently exerted a large influence on the stress-strain diagrams; one peculiarity of the brasses, however, was that the plastic line in the neighborhood of the breaking point invariably showed a slight crinkling effect notwithstanding the annealing of some of the test specimens. The curves for phosphor-bronze uniformly resembled a scythe standing on its handle. Tin, gun metal and zinc, when tested, gave characteristic diagrams. The curves for iron showed small extensions after the maximum load had been attained—cast iron showing practically no plastic extension, as might be expected. Some steels appeared to have a difficult time building up the plastic curves before they finally succeeded. In alloy steels the break at the yield point practically always disappeared—the curves in that respect having the same general character as non-ferrous metals.

A part of Professor Dally's study is of course already familiar to material engineers; the fact remains, however, that too little is known of the plastic portion of stress-strain curves for most materials, notwithstanding the fact that that, in the opinion of the writer, characterizes the metal more than does the elastic portion. It is not unlikely that as further work shows the importance of obtaining complete stress-strain curves so as to expose the past history of the material tested, and hence its probable future behavior, and as such further study permits the proper interpretation of curves, the obtaining of complete curves by rapid autographic means will be a matter for consideration as to the inclusion, at some near future date, of appropriate requirements in specifications.

To those who would object to the suggestion on the grounds of its impracticability in routine commercial testing, let them remember that in Professor Dally's tests a maximum of only about two minutes was necessary to make, through failure, most of the tests; indeed a mild steel bar (carbon 0.26 per cent) was

Mr. Lasier. broken in 10 seconds, and an iron bar in 4 seconds—both tests giving good, although somewhat ragged, autographic curves.

One further value of complete stress-strain curves will be here suggested, namely, to permit the reporting of elongation properties more precisely than by the usual method now employed. The present usual method of placing the two broken pieces of a test bar together and measuring the elongation after fracture is open to criticisms too well known to repeat here. In addition to the impossibility of securing a close contact at the plane of fracture, the extension as usually now measured consists of both a general uniform extension of the specimen as a whole and a much greater local extension in the neighborhood of the break. Professor Unwin in a paper read in 1903 before the Institution of Civil Engineers called attention to the fact that the difficulty of comparing elongations of different test bars would probably disappear if the "general elongation," as he termed it, or the elongation at the maximum load, were taken as the measure of ductility. There is no practicable way, however, of obtaining the "general elongation" except through obtaining an autographic stress-strain diagram.

Mr. Moore. MR. H. F. MOORE (*Author's closure by letter*).—Mr. Lasier's discussion points out clearly the two functions of tension tests of metals: (1) the determination of some "limit of elasticity," which fixes a limit for practical static strength of the material; and (2) the study of the plastic properties of the material, which are important in connection with the punching, shearing and cutting of the material, and also in connection with the behavior of materials under accidental overload. The apparatus described in the paper is designed for determining elastic properties only, but a combination of this apparatus and the rigging described by the writer before this Society at the 1908 meeting[1] will give both a determination of the proportional limit and a fairly correct stress-strain diagram for the whole test.

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[1] "An Autographic Recorder for Rapid Tension Testing," *Proceedings*, Am. Soc. Test. Mats., Vol. VIII, p. 653 (1908).

AN ALTERNATING TORSION TESTING MACHINE.[1]

By D. J. McAdam, Jr.

SUMMARY.

This paper is supplementary to the one published in the Proceedings last year.[2] The alternating torsion machine described in that paper has been improved by the insertion of a helical spring between the test specimen and an arm which is given reciprocating motion by an eccentric attachment to a rotating wheel. This spring was added for the purpose of automatically increasing the angle of torsion of the test specimens as the specimens yields, thus keeping the torsional moment nearly constant until just before the specimen breaks. Arrangement is also being made for automatically drawn diagrams, showing the torsional angle and two separate indications of the torsional stress.

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[1] This paper is presented by permission of the Secretary of the Navy.—Ed.

[2] McAdam, "Endurance and Impact Tests of Metals," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 292 (1916).

AN ALTERNATING TORSION TESTING MACHINE.[1]

By D. J. McAdam, Jr.

In the Proceedings of the Society for last year a description was given of an alternating torsion testing machine developed at the U. S. Naval Engineering Experiment Station, Annapolis, Md. In this article, there were also given some endurance tests on various kinds of steel. It was stated that experiments were being made in testing non-ferrous metals and alloys by the same method. In the testing of very ductile material, however, difficulties were encountered, necessitating important additions to the machine. The present article describes the modified machine.

Fig. 1 shows a sketch of the machine showing end and side views. In this figure, *K* and *J* represent the worm gear and driving wheel of the machine. The arm *B*, which is caused to oscillate by means of its eccentric attachment to the rotating wheel *A*, gives alternating rotation to a shaft passing through the bearing *C*. This shaft is attached to one end of the helical spring *D*. To the other end of the spring *D* is joined a shaft that passes through the radial and thrust ball bearing *E* and is fastened to one end of the test specimen *F*.

From the other end of the specimen *F*, a shaft passes through the radial ball bearing *G*, and is fastened to the arm *H* which extends between the springs *I*.

When, therefore, the arm *B* is caused to oscillate, alternating rotation is transmitted through the spring *D* to one end of the specimen *F*. Since this rotation is resisted by the action of the springs *I* on the arm *H*, an alternating torsion is applied to the specimen. The torsional stresses can be varied by varying the eccentric attachment on the wheel *A*.

As described in the previous paper, the torsional stresses are proportional to the displacement of the springs *I*. The amount of displacement is indicated by a magnifying pointer (not shown in Fig. 1) adjusted to the end of the arm *H*. The arrangement of the pointer and recording drum is shown in Fig. 1 of the previous paper. A counter records the number of cycles of stress applied. A comparison of Fig. 1 of this paper with Fig. 1 of the previous paper will show that the chief modification of the original machine consists in the addition of the spring *D*, the purpose of which will now be explained.

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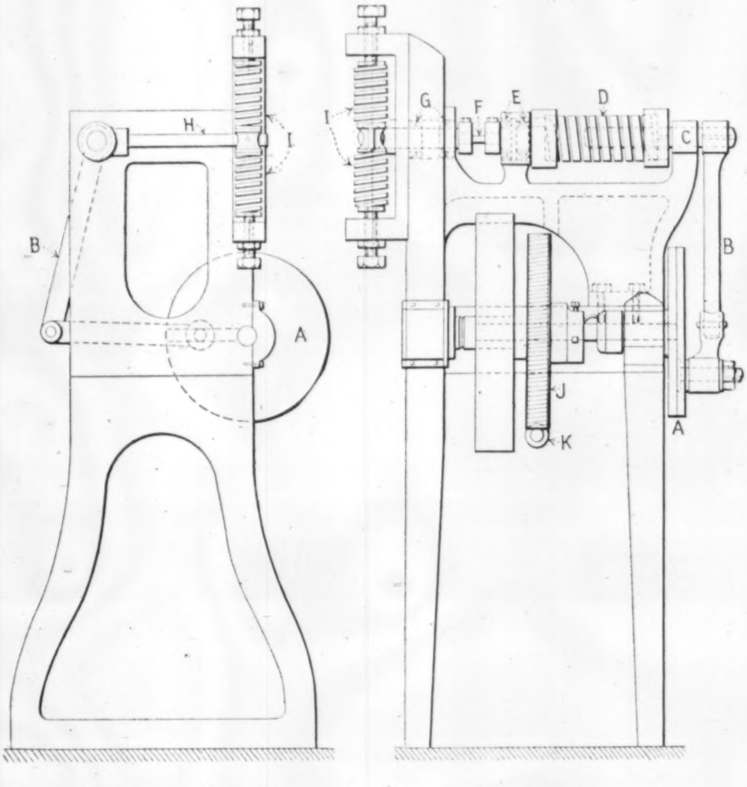
[1] This paper is presented by permission of the Secretary of the Navy.—Ed.



End Elevation. Side Elevation.

Fig. 1.—Alternating Torsion Testing Machine.

If this spring is omitted and the arm *B* connected by a shaft with the specimen *F*, the machine could be used satisfactorily in the testing of steel. For very ductile material, however, such as non-ferrous metals, the machine would not be satisfactory. As the ductile specimen yielded, the applied stress would soon drop almost to zero, and the specimen would last indefinitely unless the angle of rotation were increased. Any arbitrary increase, moreover, in the angle of rotation would, of course, make the test indefinite and worthless. The object of the spring, then, is to take up the torsional strain of the specimen as the yield takes place.

Suppose, for example, the eccentric attachment of arm *B* is adjusted so that the angle of rotation of the shaft in the bearing *C* is 5 deg. on each side of the neutral position. At the beginning of the test, the angle is usually distributed through the system so that most of it occurs in the spring *D*, while the torsional strain of the specimen *F* might be only a fraction of a degree As the specimen yields, however, the torsional strain of spring *D* decreases while that of specimen *F* increases to the maximum angle (in the supposed case, 5 deg.). By use of a long helical spring, therefore, the torsional angle of the test specimen may be automatically increased as the test proceeds, while the torsional moment is held nearly constant until a very short time before fracture occurs. As a further improvement in the use of this machine, a system of magnifying pointers is to be attached at each end of the specimen *F* and at each end of the spring *D*. By the relative motion of these pointers and by the motion of the pointer attached to the arm *H*, a complete record of the angle of torsion of the specimen, and of the torsional moments applied by springs *D* and *I*, may be obtained.

Since the above described improvements have been but recently added to the machine, very few tests on non-ferrous metals have been completed. Preliminary experiments, however, have shown that the machine in its improved form will be useful in testing the toughness of non-ferrous metals as well as of steel.

A NEW CONSISTENCY TESTER FOR VISCOUS LIQUID BITUMINOUS MATERIALS.

By Prévost Hubbard and F. P. Pritchard.

SUMMARY.

Many petroleum and tar products, especially those used in the hot-surface treatment of highways, are of such consistency that at normal temperature they cannot well be tested with the instruments ordinarily employed for determining the consistency of bituminous road materials. The Engler viscosimeter is, however, commonly employed for such products by making the test at a sufficiently elevated temperature to insure proper fluidity. Such a test does not accurately indicate viscosity or consistency at normal temperature and may be extremely misleading if an attempt is made to so interpret it. This fact is demonstrated by temperature viscosity curves for various bituminous road materials.

In attempting to devise a convenient laboratory instrument for determining consistency at normal temperatures the authors have followed the general principle of the Lunge and Hutchinson tar testers, with a view to improvement in certain important details. After numerous trials an instrument was obtained made of aluminum and weighted with lead shot. This instrument is 3 in. long and weighs exactly 2.8 g. It consists of two aluminum rods of different diameters, one of which is hollow, a thin aluminum disk 1.25 in. in diameter and a tapered bottom weight. The larger rod is about one-half the length of the smaller which passes through it and screws into the bottom weight. The aluminum disk is riveted transversely to the rods where they join about midway of the length of the instrument thus forming a balanced plummet.

A special bitumen container is used with this instrument. It consists of a cylindrical flanged copper cup 2 in. in diameter and

$3\frac{3}{4}$ in. deep, which is placed in a water-bath fitted with a stirrer passing through the flange of the cup. The flange is also provided with a small hole for the insertion of a thermometer in the bath.

Before making a test, the bituminous material which fills the inner compartment of the water-bath, is brought to a temperature of exactly 25° C. which must be maintained in the bath throughout the test. The surrounding atmosphere should also be as close to 25° C. as possible. The test is made by allowing the instrument to sink of its own weight in the bituminous material from the upper edge of a lower scale marking on the small rod to that of an upper marking, and noting the time required.

This instrument was found to have a wide range of usefulness as shown by numerous tabulated test results. At normal temperatures it may be used for all bituminous materials outside of the range of the Engler viscosimeter and the usual penetration machine. When used in connection with a representative set of commercial bituminous road materials, it was found that comparatively few materials whose penetration at 25° C. could not be ascertained required more than 30 minutes for a test with the new instrument. In fact, most of the material prepared for hot-surface treatment, showed a test of less than 15 minutes.

While bringing the new instrument to the attention of those particularly interested in the testing of bituminous road materials, the authors also desire to call attention to its possible usefulness as a means of controlling and specifying the consistency of paints and other viscous fluid materials.

A NEW CONSISTENCY TESTER FOR VISCOUS LIQUID BITUMINOUS MATERIALS.

By Prévost Hubbard and F. P. Pritchard.

Introduction.

Many petroleum and tar products, especially those used in the hot-surface treatment of highways, are of such consistency that at normal temperature they cannot well be tested with either of the two instruments ordinarily employed for determining the consistency of bituminous road materials. These products should be classed as liquid bituminous materials according to the standard definition of the American Society for Testing Materials, as under the penetration test they show a penetration of more than 350 at 25° C. (77° F.), under a load of 50 g., applied for one second.

The standard penetration test is restricted to determinations of the consistency of semi-solid and solid bituminous materials, as a penetration of 350 (3.5 cm.) of a standard needle under a load of 50 g., applied for one second constitutes the practical working limit of the penetration machine for soft materials.

For determining the consistency of most liquid bituminous road materials the Engler viscosimeter is ordinarily employed, and consistency thus determined is expressed as so-called "specific viscosity." While the Engler instrument is reasonably satisfactory for the products which flow readily at normal temperature, its practical working limit at that temperature does not include many of the very viscous bituminous materials which are too soft for a penetration test. It has, however, been widely used in connection with such materials by making the test at a sufficiently elevated temperature to insure a degree of fluidity in the material which will come within the practical working range of the instrument. The higher temperatures most commonly employed are 50° C. and 100° C., although 40° C. is also used to some extent.

In the laboratory of the U. S. Office of Public Roads and Rural Engineering it has been the practice to consider a specific viscosity of 200 about the practical working limit of the Engler viscosimeter, although if the room temperature is materially lower than that of the bitumen, the rate of flow is not sufficient to prevent slight congealing at the end of the outlet tube, which in some cases introduces a serious error in results.

Moreover, if it is desired to control or ascertain the consistency of a material at normal temperature, tests made at elevated temperatures are apt to be extremely misleading when comparing different types of bituminous materials. The temperature viscosity curves for even the same general type of material are not always identical, although they may coincide at a selected test temperature. These facts are illustrated by test results shown in Figs. 1 and 2, although for reasons above mentioned it is impracticable to show in the diagrams the exact specific viscosity of all the materials at 25° C. The same objection holds true with respect to any other instrument, such as the penetrometer or float apparatus when it is desired to ascertain consistency at normal temperature by a test made at some other temperature. In addition to the above, many of the bituminous materials

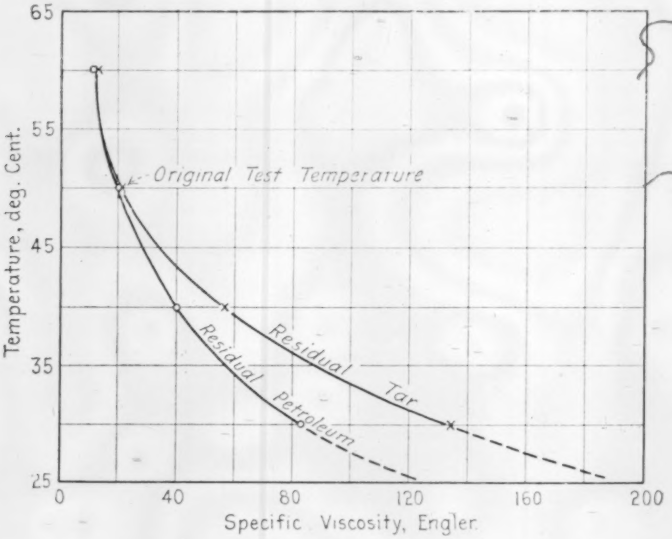


FIG. 1.

contain highly volatile constituents which evaporate rapidly at even slightly elevated temperatures and thus produce changes in viscosity during the test itself.

PRELIMINARY EXPERIMENTS.

In attempting to devise a simple laboratory instrument for determining the consistency at normal temperature of viscous liquid bituminous materials, the authors followed the general scheme embodied in Lunge's[1] tar tester and later improved by Hutchinson[2] in the Hutchinson tar tester.

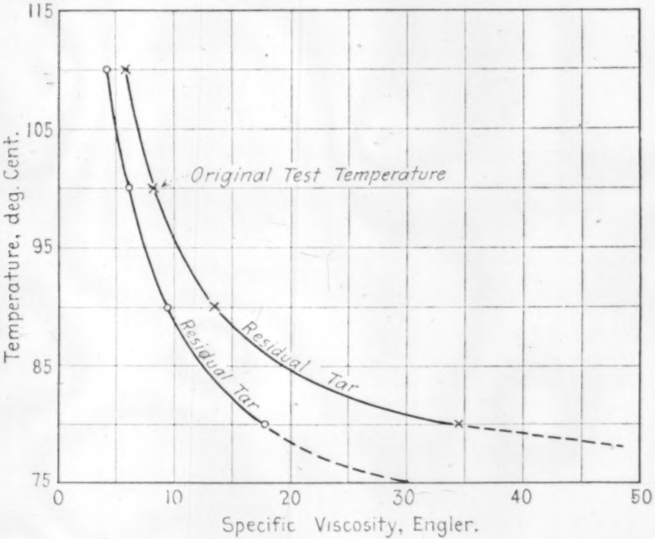


FIG. 2.

The Lunge tar tester is made of glass and in size and shape resembles a petroleum hydrometer. It is 32.5 cm. (12.8 in.) long, and in making a test is allowed to sink for a specified distance in the bituminous material contained in a tall glass cylinder. The time required for the instrument to sink to a given mark when dropped into a sample of the material is recorded as a measure of the consistency of the material. This instrument is very readily broken when it is being cleaned; the glass cylinder is difficult to clean quickly; no accurate control of the temperature of the sample is possible without an unwieldy and specially designed constant temperature bath; and too large a sample is required for the test.

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[1] George Lunge, "Coal Tar and Ammonia," Fifth Edition, p. 551 (Gurney and Jackson).

[2] British Patent 22042 of 1911.

The Hutchinson tar tester, which is a much more serviceable instrument, is made of German silver. It consists of a straight metal rod about 9 in. long, to the lower end of which is attached a bullet-shaped poise. About midway of the rod is a flat double cone-shaped air-tight bulb, which serves to retard the passage of the instrument through the bituminous material. Above the bulb and on the rod are two rings about 2 in. apart. In making a test the instrument is immersed in the material to the lower ring and the time required for it to sink to the upper ring is recorded as a measure of the consistency of the material. A cylindrical vessel about 9 in. deep and 4 in. internal diameter is used for a container. While if properly used this tar tester is an instrument of precision, no accurate control of the temperature of the sample is possible without an unwieldy and specially constructed constant temperature bath; too large a sample is required for a laboratory test; and cleaning the instrument and container is a rather messy proposition when very viscous materials are tested.

In undertaking the work of developing an instrument for the measurement of relative consistencies, it was first necessary to construct a crude apparatus to give some idea of test values. For this purpose a circular aluminum disk, 1 in. in diameter, was fastened transversely on the middle of a small glass rod 4 in. long, and the time required for the apparatus to sink a definite distance into various liquid bituminous road materials was noted. This showed that, while an apparatus of such weight could be used in the heavier fluid bitumens, it would not give readable results on materials whose viscosities were obtained in the Engler viscosimeter at 25° C.

An attempt was then made to retard the rate of sinking by using two disks instead of one, but this was abandoned as unsatisfactory. After many trials it was found that a small aluminum rod, weighted at the bottom, and carrying a very thin disk transversely at a suitable place along its axis, could be used successfully to cover a wide range of materials, from a thin, watery fluid, to a substance having at 25° C., under a load of 50 g. for one second, a penetration of about 300. The details of design were then worked out by experimenting with different combinations of bottom weights and disks.

Three combinations were finally obtained which gave reasonably satisfactory results on a fairly wide range of materials. The one selected as being most compact and convenient to use was about 3.5 in. long. It consisted of an aluminum rod 0.04 in. in diameter, to which was screwed a conical steel bottom weight 0.236 in. by 0.4 in., weighing 0.85 g., and 2 in. from the bottom of which was riveted a circular aluminum disk 1.25 in. in diameter by 0.008 in. thick. The disk was perforated with vent holes

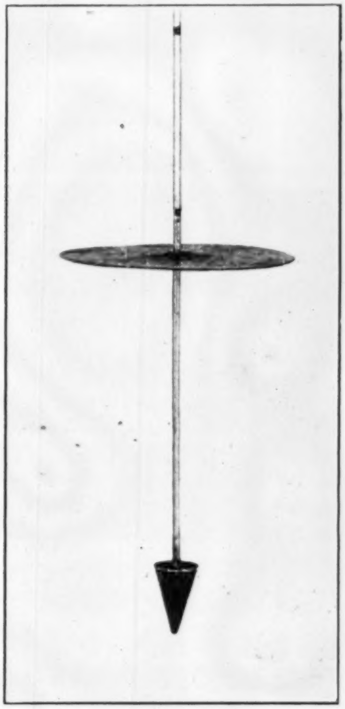

FIG. 3.—First Consistency Tester.

0.04 in. in diameter on opposite sides, 0.0625 in. from the center of the rod. The upper end of the rod, extending 1.5 in. above the disk, was divided into scale markings, one 0.25 in. and the other 1.25 in. above the disk, so that the distance from the upper edge of the lower marking to upper edge of the upper marking was exactly 1 in. The instrument assembled weighed 1.5 g. in air and 1.16 g. in water, with a specific gravity of 4.43.

A number of these instruments were made at the U. S. Bureau of Standards under specifications prepared by the authors. The aluminum was drawn or rolled to proper diameter or thickness, gages were turned out to aid in the making of the steel weights, and dies were constructed for assembling the disk, rivet and rod. The rods were cut to proper length and scale markings accurately machined thereon. The scale markings were then filled with red baking enamel and heated for several hours at 80° C. The bottom weights were turned and cut off a trifle long, and filed to size in a gage prepared for that purpose. They were then drilled, tapped, and screwed in place.

TABLE I.—RESULTS OF TESTS USING FIRST CONSISTENCY TESTER.
VALUES ARE TIME IN SECONDS.

Material.	Instrument No.							
	1	2	3	4	5	6	7	8
Texaco Road Oil	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5
Texaco Road Oil	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6
Tarvia B	1.9	1.9	1.9	1.9	1.9	2.0	2.0	2.0
Standard Road Oil	3.0	3.0	2.9	3.0	3.0	3.0	3.0	3.0
Texaco Surfacing Compound	6.0	6.1	6.0	6.1	6.2	6.0	6.0	6.0
Standard Liquid Asphalt	6.1	6.1	6.0	6.2	6.0	6.0	6.0	6.0
Ugite B	10.6	10.4	10.3	10.4	10.5	10.4	10.4	10.5
Texaco Liquid Asphalt	14.5	14.4	14.4	14.4	14.4	14.4	14.6	14.4
Standard Road Oil	39.4	41.0	38.5	40.0	41.0	39.0	39.0	40.0
Tarvia A	685.0	670.0	680.0	675.0	677.0	676.0	675.0	685.0
Standard Flux	1925.0	1902.0	1920.0	1905.0	1900.0	1910.0	1905.0	1935.0

This instrument, shown in Fig. 3, was found to have a wide range of usefulness, giving a test of 0.4 second for a material with a specific viscosity (Engler) of about 6, and a test of two and one-half hours for a material of about 300 penetration under a load of 50 g. applied for one second. When used in connection with a representative set of commercial bituminous road materials, it was found that comparatively few materials whose penetration at 25° C. could not be ascertained required more than 30 minutes for a test with the new instrument. In fact, most of the material prepared for hot-surface treatment showed a test of less than 15 minutes. Moreover, some eight instruments, which were made for the purpose, were found to give practically identical results, as shown in Table I.

In view of the fact that the Engler viscosimeter has been so widely used as a means of determining the consistency of liquid bituminous materials, it was thought that it would be interesting and useful to determine what relation, if any, existed between test values obtained with the Engler instrument and with the instrument just described. Without entering into great detail it may be stated that for the purpose of comparing the viscosities of liquids differing materially in specific gravity, the Engler viscosimeter has always been of questionable value when the test is made in the ordinary manner with the same initial volume of the liquid under test. Under such conditions it is evident that the static pressure over the outlet tube must vary with the specific gravity of the material so that the factors controlling the rate of flow must also vary with the specific gravity. This means that two materials of the same absolute viscosity might show widely different Engler viscosities if their specific gravities differed materially. This is an important matter, when it is considered that liquid bituminous road materials may vary in specific gravity from less than 0.9 to somewhat over 1.2.

TABLE II.—RESULTS OF TESTS WITH ENGLER VISCOSIMETER.

	Amyl Alcohol 15° C.	30-per-cent Cane Sugar Solution 20° C.
Specific gravity	0.816	1.1285
Absolute viscosity	309	307
Engler viscosity first 50 cc. (240-cc. sample), seconds	16.0	14.0
Engler viscosity first 50 cc. (271-g. sample), seconds	14.2	14.0

To demonstrate this error in the ordinary use of the Engler viscosimeter a search was made for two liquids of approximately the same absolute viscosity which differed materially in specific gravity. Amyl alcohol and a 30-per-cent solution of cane sugar were found to answer this purpose, as the former was found to have an absolute viscosity of 309[1] at 15° C. and the latter 307[1] at 20° C. The specific gravity of amyl alcohol at 15° C. is 0.816, while that of a 30-per-cent solution of cane sugar at 20° C. is 1.1285. The time in seconds required for the first 50 cc. of a 240-cc. sample of each liquid at temperatures of 15° C. and 20° C.

TABLE III.—COMPARISON OF ENGLER VISCOSIMETER AND FIRST CONSISTENCY TESTER AT 25° C.

Material.	Specific Gravity 25°/25° C.	Engler Viscosity at 25° C. First 50 cc.			Experimental Instrument at 25° C., time, sec.
		240-cc. Sample.		240 g.	
		Seconds.	Specific Viscosity.	Specific Viscosity.	
Texaco No. 30 Road Oil	0.907	63	5.7	5.1	0.4
Texaco No. 40 Road Oil	0.940	365	33.2	31.4	1.6
Tarvia B	1.152	381	34.6	36.8	2.2
Standard Asphalt Road Oil	0.934	780	70.9	66.2	3.2
Standard Liquid Asphalt	0.942	1387	126.1	121.2	7.1
Texaco Surfacing Compound	0.934	2091	190.1	179.6	10.2
Ugite B	1.122	2190	199.1	204.6	11.4
		Engler Viscosity at 50° C.			
Texaco Liquid Asphalt No. 1	0.968	625	56.8	55.0	18.8
Standard Road Oil No. 6	0.959	727	66.1	63.1	43.6
		Engler Viscosity at 100° C.			
Tarvia A	1.218	57	5.2	6.1	756 sec. = 12 min. 36 sec.
Texaco Special Macadam Binder	1.006	497	45.2	47.6	2225 sec. = 37 min. 5 sec.
Texaco Macadam Binder	1.012	870	79.1	82.6	8880 sec. = 2 hr. 28 min.
		Penetration at 25° C., with 50-g. weight 1 sec. = 305 mm.			

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[1] *Chemiker Kalendar*, 1909, p. 328.

respectively to be discharged from the Engler viscosimeter was found to differ appreciably as shown in Table II. When, however, equivalent weights of the liquids were tested under the same conditions, the Engler viscosities were found to coincide almost as closely as the absolute viscosities.

While these results may not be considered conclusive, they nevertheless bear out the theory that equivalent weights rather than equivalent volumes should govern the charge in the Engler viscosimeter. In determining specific viscosities by means of the Engler apparatus, it would therefore appear advisable to limit the test sample to 240 g. (the weight of 240 cc. of water) rather than to 240 cc. When this was done some interesting relations between the Engler viscosimeter and the instrumen previously described were developed. These are shown in

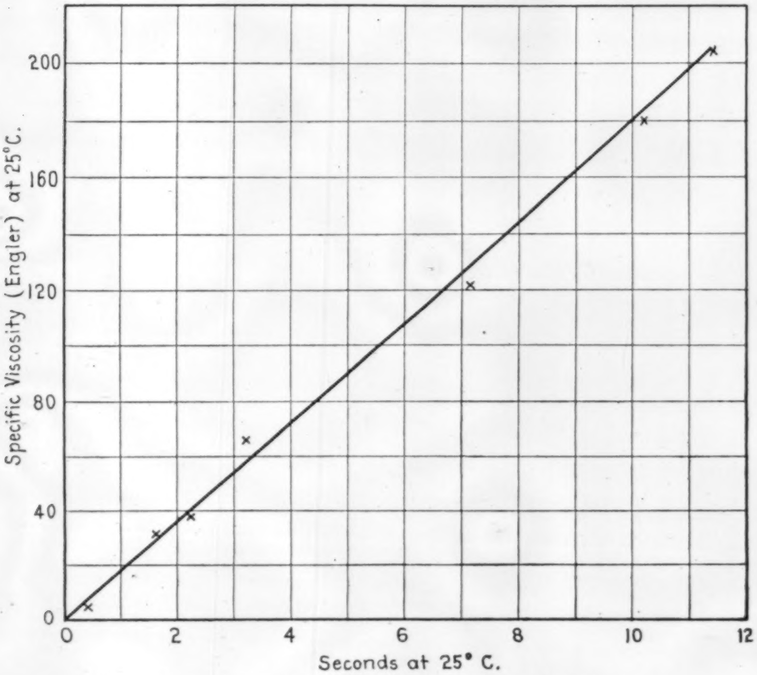

FIG. 4.—Tests with First Consistency Tester.

Table III, which also illustrates the wide range of usefulness of the instrument.

Considering the viscosity tests at 25° C., it will be noted that a much more definite relation exists between the results obtained with this instrument and the viscosity test on 240 g. of material than between the instrument and the viscosity test made on 240 cc. of material. When plotted as shown in Fig. 4, this relation to all intents and purposes takes the form of a straight line. For reasons previously discussed there can of course be no definite general relation between specific viscosities at elevated temperatures and the results obtained with the instrument at 25° C.

DESCRIPTION AND USE OF NEW CONSISTENCY TESTER.

While this instrument produced satisfactory results, in so far as indicating the relative consistency of fluid bituminous materials was concerned, experience proved that it was too delicate for rough routine laboratory work, as it was readily bent and twisted out of shape. Modifications in its construction were therefore necessary in order to produce a stouter instrument which would withstand ordinary laboratory handling without injury.

This modified apparatus, shown in Figs. 5 and 6, was made at the Bureau of Standards. It is 3 in. long, weighs 2.8 g. in air and 1.487 g. in water, and has a specific gravity of 2.13. The circular aluminum disk *A*, 1.25 in. in diameter and 0.008 in. thick, is riveted to the middle of the aluminum rod *B* which is 0.625 in. in diameter. It is also riveted to the end of the hollow aluminum rod *C*, 0.25 in. in diameter and 1.3125 in. long. The upper end of rod *C* which is solid except for a hole large enough to allow entry of the small rod *B* is turned off to form a square shoulder. This end is passed through a snug-fitting hole in the center of the disk and is used to rivet the disk to the two rods at the shoulder. The disk carries two holes 0.04 in. in diameter, placed on opposite sides 0.145 in. from the center. The lower end of rod *B* is threaded and screws into the conical aluminum bottom weight *D*. The bottom weight has a 0.1875-in. taper running to the same diameter as rod *C*. Its upper end is turned off so as to fit snugly into the hollow end of rod *C* when it is screwed to rod *B*. The upper end of rod *B* carries two scale markings filled with red baking enamel, one 0.25 in. and the other 1.25 in. above the disk. The upper edges of these markings are exactly 1 in. apart. The instrument, which weighs normally 1.8 g., is filled with fine lead shot to a weight of exactly 2.8 g.

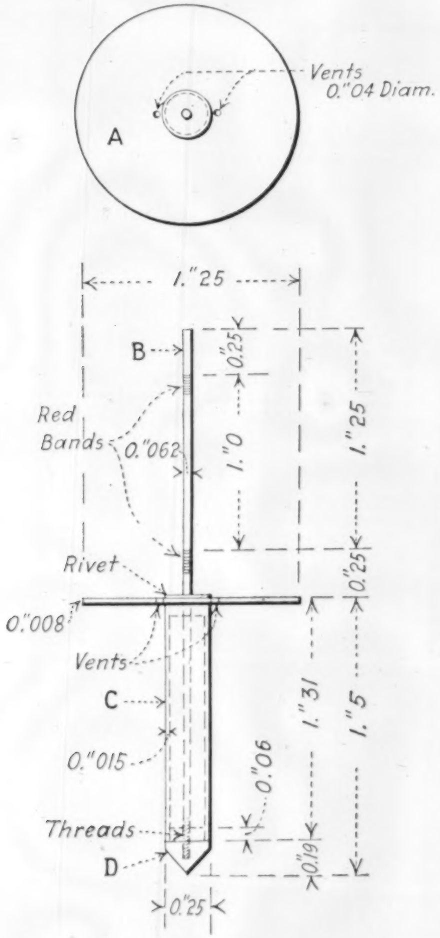
Vents
0."04 Diam.
A
1."25
B
0."25
Red
Bands
0."062
1."0
1."25
Rivet
0."25
0."008
Vents
C
1."31
1."5
0."015
0."06
Threads
D
0."19
0."25

FIG. 5.—Details of New Consistency Tester.

A special bitumen container, shown in Fig. 7, is used with this instrument. It consists of a cylindrical flanged copper cup

A, 2 in. in diameter and 3.75 in. deep, which is placed in the water-bath *B* fitted with a stirrer *C* passing through the flange of cup *A*. The flange is also provided with a small hole for the insertion of a thermometer in the bath.

Before making a test, the bituminous material, which fills the inner compartment of the water-bath, is brought to a temperature of exactly 25° C. which must also be maintained in the bath throughout the test. The surrounding atmosphere should

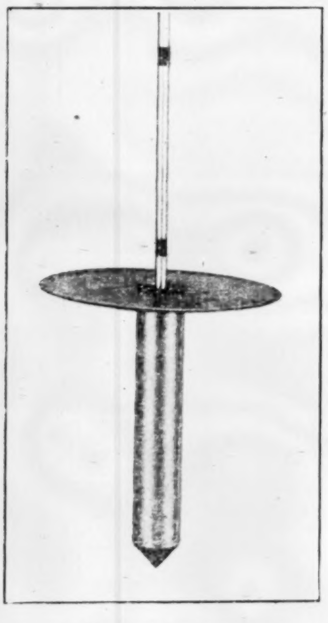

FIG. 6.—New Consistency Tester.

also be as close to 25° C. as possible. When filling the container it is important that no air bubbles be imprisoned in the bituminous material, as they are apt to materially retard the passage of the instrument. Viscous materials should be warmed before pouring them into the container and should then be allowed to cool to 25° C. before making the test. If air or gas bubbles rise to the surface, they should be removed from time to time with a tiny flash point jet until the surface of the material remains perfectly smooth.

The test is made by allowing the instrument to sink of its own weight in the bituminous material from the upper edge of the lower scale marking to that of the upper marking, and noting the time required. When placing the instrument in the material, the disk should first strike it at an angle and then be permitted to settle until it lies flat. This precaution eliminates the chance

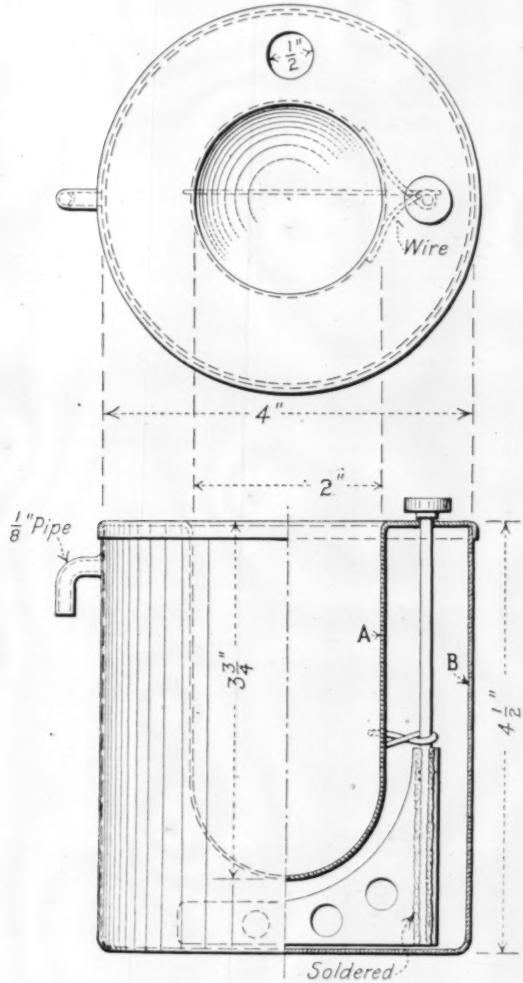

FIG. 7.—Bitumen Container.

TABLE IV.—RESULTS OF TESTS USING NEW CONSISTENCY TESTER, SHOWING RANGE OF USEFULNESS.

VALUES ARE TIME IN SECONDS.

Material.	Instrument No.					
	Operator A.		Operator B.		Operator C.	
	1	2	3	4	5	6
Texaco Road Oil	0.5	0.5	0.5	0.5	0.6	0.6
Texaco Road Oil	1.2	1.1	1.3	1.1	1.3	1.3
Tarvia B	2.0	2.0	1.9	1.8	2.2	2.2
Standard Road Oil	2.5	2.6	2.6	2.6	2.4	2.4
Ugite B	8.9	9.3	8.7	8.8	9.4	9.4
Texaco Surfacing Compound	11.6	11.2	11.1	11.1	11.4	11.2
Texaco Liquid Asphalt	13.0	13.8	13.4	13.4	13.6	13.4
Standard Road Oil	29.8	31.4	30.8	29.8	29.4	29.0
Tarvia A	780.0	835.0	815.0	830.0	800.0	838.0
Texaco Special Macadam Binder	42 min. 40 sec.	45 min. 0 sec.	42 min. 15 sec.	45 min. 0 sec.	42 min. 42 sec.	42 min. 10 sec.
Texaco Macadam Binder	2 hr. 24 min.	....	2 hr. 26 min.	....	2 hr. 27 min.	....

TABLE V.—COMPARISON OF ENGLER VISCOSIMETER AND NEW CONSISTENCY TESTER AT 25° C.

Material.	Engler Viscosity at 25° C. 240-g. sample, first 50 cc.		New Instrument; Time at 25° C., sec.
	Time, sec.	Specific Viscosity.	
Texaco No. 30 Road Oil	63	5.7	0.5
Texaco No. 45 Road Oil	413	35.7	1.3
Texaco No. 55 Road Oil	450	41.0	1.4
Tarvia B	471	42.8	2.1
Standard Asphalt Road Oil	888	80.7	2.7
Trinidad Liquid Asphalt A	1238	112.5	3.8
Standard Liquid Asphalt	1350	122.7	4.1
Aztec Road Oil	1725	156.8	5.1
Aztec Road Oil	1762	160.2	5.7
Ugite B	44 min. 30 sec.	242.7	9.2
Texaco Surfacing Compound	1 hr. 27 min.	474.5	14.6

of imprisoning air under the disk. In the case of very viscous materials, when it is necessary to push the disk below the surface, the top of the lower scale marking should never be lower than the general level of the material in the container, when the actual test is started. Any surface deformation due to such procedure should first be allowed to correct itself. After a test has been made the instrument should be removed and allowed to drain. It should then be immersed in a beaker of solvent for a few minutes and then carefully dried. Gas holder oil, carbon disulfide or solvent naphtha will be found convenient

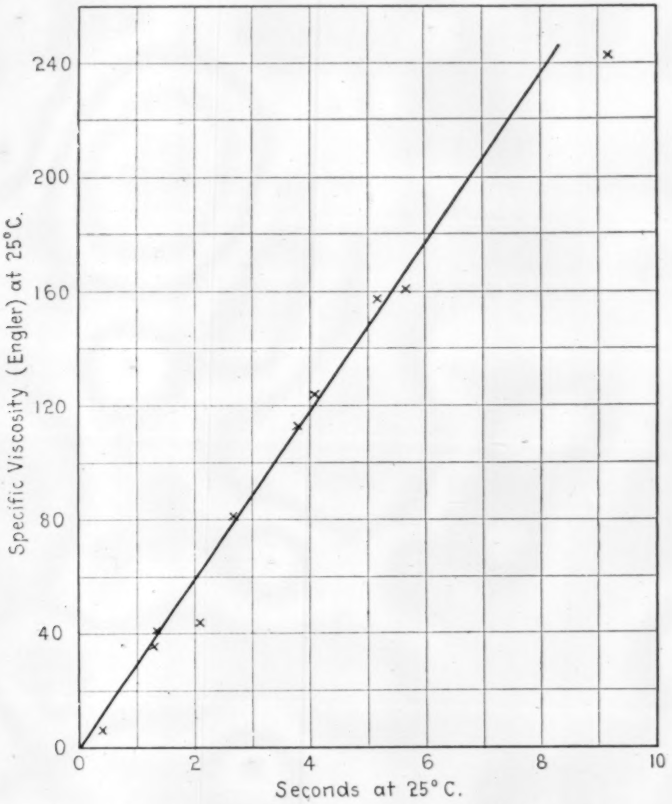

FIG. 8.—Tests with New Consistency Tester.

for use as a cleaning solvent. If the material is very viscous, the instrument may be allowed to drain in an oven at about 100° C. for a few minutes before immersing it in the solvent. As it is made of aluminum it should of course never be heated in a free flame.

The range of usefulness of this new instrument was found to be quite similar to that of the one previously described as shown by a comparison of Tables I and IV. Table IV also illustrates the ordinary limits of accuracy as determined by three operators working with six different instruments on the same bituminous material.

Table V gives comparative results at 25° C. obtained upon the same bituminous materials with the Engler viscosimeter and the new instrument.

By plotting the specific viscosity against the time in seconds of the tests with the new instrument as shown in Fig. 8, it is seen that practically a straight line curve is obtained within the working range of the Engler instrument, which is comparable with, but inclines at an angle different from that of the curve for the first instrument devised.

While bringing the new instrument to the attention of those particularly interested in the testing of bituminous road materials, the authors also desire to call attention to its possible usefulness as a means of controlling and specifying the consistency of paints and other viscous fluid materials.

DISCUSSION.

Mr. Herschel.

MR. W. H. HERSCHEL.—There is a certain gap between material whose hardness can be determined with a Brinell hardness machine and whose viscosity can be determined with a viscosimeter, which it is desirable to fill. The instrument described by the authors is one of the means for filling this gap and is therefore very interesting. There are, however, certain points in this paper, to which exception could be taken. For example, it is stated that 200 is considered the practical limit for the Engler viscosimeter, whereas in Technical Paper No. 74 of the Bureau of Mines on the properties of the petroleums of California, viscosities are given up to 5499 Engler degrees. You will note that I say Engler degrees and not specific viscosity. I think the term "specific viscosity" should be avoided, because the impression is given that specific viscosity means relative viscosity, whereas it is nothing of the sort. Engler degrees simply express the ratio between the time of flow for the substance being tested and the time of flow for a very poorly chosen calibrating liquid, namely water, and specific viscosity is not relative viscosity.

In Table II the absolute viscosity of amyl alcohol is given as 309, which is evidently intended for 0.0309 c.g.s. units. Thorpe and Rodger[1] found that the viscosity of amyl alcohol varied greatly in different samples, the lowest value obtained being about 0.0517 at 15° C. Using Eq. 9 in my paper on absolute viscosity,[2] the following table may be obtained.

Liquid.	Viscosity.	$\frac{\text{Viscosity}}{\text{Density}} =$ Kinematic Viscosity.	Time, Engler with Normal Filling, sec.	Time for 240 cc. Put In and 50 cc. Run Out, sec.	Ratio, Last Two Columns.
Amyl alcohol......................	0.0309	0.0379	65.0	16.0	4.1
" "	0.0517	0.0634	76.4	16.0	4.8
30 per cent sugar..................	0.0307	0.0273	60.6	14.0	4.3

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[1] *Philosophical Trans.*, Royal Soc., Vol. 185, Part II, p. 572 (1894).

[2] "Determination of Absolute Viscosity by the Saybolt Universal and Engler Viscosimeters," p. 551.—ED.

Mr. Herschel. In tests which I have made with ethyl alcohol and water solutions, the ratio of time, Engler, with 200 cc. run out, and time for 50 cc., was found to lie between 4.5 and 4.7, the value decreasing somewhat with the viscosity. These values of the ratio roughly check the last column of the table. A more accurate check is not to be expected on account of the shortness of the time of flow and the uncertainty in regard to the viscosity of the amyl alcohol.

It is not certain whether the tests of Table II were made with the instrument filled up to the gage points, it being assumed that this would require 240 cc.; or whether 240 cc. was measured out and poured into the instrument. This latter method was originally used by Engler.[1] However, on April 1, 1907, the Reichsanstalt and other German testing laboratories came to an agreement to discard the value of 240 cc. entirely and use the gage points.[2] This is the preferable method, because the volume of the dished part of the container—that is, the capacity of the container when filled to the gage points, less the 200 cc. to be withdrawn—can have no effect on the head causing flow. Moreover, since the contour of the bottom of the container is not specified, it differs in different instruments, with a consequent variation in the volume required to fill the container up to the gage points.

By means of Table II of the paper an attempt is made to prove that weight instead of volume should be the controlling factor in filling the Engler instrument. As a matter of fact, volume is not the controlling factor and weight should not be either. There are very few instruments, probably, that could be filled up to the gage point with 240 cc. of water. Every instrument I ever tested required more than 240 cc. Moreover, there is no ambiguity whatever due to the fact that two liquids of different densities but the same viscosity will have different times of discharge, because density is taken care of in the equation. A viscosimeter of the Engler or Saybolt type does not measure absolute viscosity and should not be expected to. All it measures is what is called kinematic viscosity, namely, the ratio between the absolute viscosity and the density; therefore there is no

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[1] *Zeitschrift für angewandte Chemie*, Vol. 5, p. 725 (1892).

[2] Holde-Müller, "The Examination of Hydrocarbon Oils," 1915, p. 105.

Mr. Herschel. ambiguity introduced by the fact that the time of flow may be different for two liquids of the same absolute viscosity if the density is different. It only adds confusion and introduces a new source of error to propose the use of a constant weight in a viscosimeter instead of a constant volume, whereas the constant volume is already out of date and the constant weight never should have been thought of.

Mr. Dow. MR. A. W. DOW.—I want to make a few remarks on Mr. Hubbard's interesting test and also to call attention to another test which I have been using in my laboratory this past fifteen years for testing the consistency of road oils, tarvias and such materials. This apparatus was briefly described in my paper "The Relation Between Some Physical Properties of Bitumens and Oils," published in the Proceedings of this Society.[1] No special modifications have been made to the apparatus since then with the exception of making two additional plungers of different sizes. The apparatus consists of a cylinder 2 cm. in diameter and 9 cm. long. This cylinder is fastened in the center of a brass cup which acts as a water bath to maintain the material being tested at a standard temperature. There are three inverted cone plungers, respectively 1.95, 1.90 and 1.85 cm. in diameter. In the center of these plungers is fitted a brass rod about 6 cm. long. The apparatus is meant as an accessory to the Dow penetration machine, a description of which will be found in the Proceedings of the Society.[2] The brass rod of the plunger is fitted in the place of the needle holder and the test is made by ascertaining the distance the plunger will sink in the material under test in the cylinder under a given weight and in a given time. With plungers of three different sizes I have found it possible to determine the consistency of liquids ranging from water to the heaviest road oils. There have been several of these instruments made and furnished to different oil companies and laboratories, but as the Engler viscosimeter has been so generally used and specified for determining the consistency of road oils, I have not pushed the introduction of this apparatus. It has several advantages, however, over any of the viscosimeters: the test is immaterially affected by the specific

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[1] *Proceedings*, Am. Soc. Test. Mats., Vol. VI, p. 501 (1906).

[2] Dow, "The Testing of Bitumens for Paving Purposes," *Proceedings*, Am. Soc. Test. Mats., Vol. III, p. 349 (1903).

Mr. Dow. gravity of the oil; only about 50 grams of the material are necessary for making the test; and the same sample can be retested as often as desirable at intervals of a few seconds, or only as long as it takes to raise the plunger in the oil.

The cylinder can be removed from the water jacket by merely a twist, which unlocks it from the bottom. In this way several cylinders can be used and tests on a number of oils made at short intervals.

An instrument resembling Mr. Hubbard's very closely was devised by myself a number of years ago and included in the patent of Mr. H. C. Bowen on an apparatus for determining viscosity, No. 49474 and dated 1893. This consisted in using a disk in place of the needle in the Bowen penetration machine. The rapidity with which the disk sank in the oil when attached to this apparatus indicated the consistency of the material under test. This patent was assigned to the Barber Asphalt Paving Co. and the apparatus was used in their laboratory for testing the consistency of heavy asphaltic fluxes. As this test was of no particular value in those days it was but little used.

I wish to take exception to one point in the paper. There is little or no relation between his test on heavy oils and the penetration test on asphalt. One of the principal factors brought out by the penetration test on asphalts is the surface tension of the material. In the case of Mr. Hubbard's apparatus the surface tension has little or no effect upon the test. This point is also covered in my paper on "The Relation Between Some Physical Properties of Bitumens and Oils," previously referred to.

Mr. Church. MR. S. R. CHURCH.—There is a real need for a simple test by means of which the consistency of highly viscous bituminous materials may be easily determined. Tests made in our laboratory confirm the authors' general results with the Lunge and Hutchinson instruments. The test for viscosity as determined by the Engler viscosimeter is a difficult and tedious one when applied to a certain class of viscous bitumens, and serious inaccuracy is often encountered, especially when the bitumens contain an appreciable amount of insoluble material.

It should perhaps be emphasized that the range of the instrument proposed by the authors is limited. It is quite evident from a consideration of the time required for determining the consistency of material suitable for cold application, that is, from less than one second to about $2\frac{1}{2}$ seconds, that such a short time interval is not sufficient to render the test a valuable control for materials of that character. Its particular field would seem to be for testing materials of the consistency ordinarily used for hot surface treatment. **Mr. Church.**

MR. PRÉVOST HUBBARD.—Replying to Mr. Herschel, I wish to call attention to the fact that we realize the validity of his contention that specific viscosity is a misnomer. In fact, on the first page of our paper we have mentioned it as "so-called" specific viscosity. However, the term specific viscosity has been so widely used now among road engineers in specifications for this class of product that it is almost impossible to discuss the subject so that they will understand it without making use of the term, and hence it was used in the paper. **Mr. Hubbard.**

As to the practical working limits of the Engler viscosimeter being 200 so-called specific viscosity, we had reference to the commercial practical working limit, not to the actual. Of course there is probably no actual limit if all the test conditions are accurately maintained for a sufficient length of time, but there is a practical commercial working limit which is a purely arbitrary matter to be decided upon by the testing engineer. In our case we found that it was inadvisable to use more time and attempt to get closer control of test conditions than would be demanded by a test of 200, particularly if the outside or air temperature was materially different from the test temperature. This is due to the fact that if the air is cooler than the test temperature there is a tendency for very slow flowing material to congeal at the outlet of the tube and thus retard flow.

In connection with the relation of volume to weight, I do not wish to enter into any theoretical discussion of the subject, as Mr. Herschel is much better posted on the purely theoretical considerations; but I wish to state that the constant weight of the sample tested appears to us to be more rational than a constant volume. Certainly the relations between tests obtained on a constant weight and a constant volume basis as compared with tests of the instruments which we used, show that there is a more definite relation to the former. Mr. Herschel has stated that the volume is not a proper standard, and this of course

Mr. Hubbard. applies to wherever the gages are set, irrespective of whether they measure 240 cc. or more. In fact one of the viscosimeters we use has its gage set for more than 240; another quite close to 240.

In connection with Mr. Dow's remarks, I agree with him that there is some difference between the character of the test described in our paper and the character of a penetration test, due to surface tension of the material, because in the penetration test the needle or the plunger is not inserted in the material before the test is begun but is brought in contact with the surface. However, there is a closer relation, I believe, between our test and a test made with his instrument, because the effect of opposing surfaces has been eliminated; in other words, the film of material between the plunger which Mr. Dow uses and the container is so slight that the effect of adhesion on opposing surfaces is obtained, which increases any difference in connection with the penetration test.

METHOD FOR STUDYING THE EFFECTS OF TEMPERATURE UPON THE PHYSICAL CONDITION OF ASPHALTS, WAXES, ETC.

By J. A. Capp and F. A. Hull.

SUMMARY.

The test specimen is a wedge-shape piece supported as a cantilever beam, in a heating chamber the temperature of which is gradually and uniformly raised. The manner and temperature of distortion of the test wedge is noted. Some waxes and simple bitumens melt throughout and drop off practically as though sheared at the support. Others bend under their own weight through large angles without material change in shape before finally stringing out and dropping.

The test method yields valuable indications of the behavior of waxy and bituminous materials over a wide range of temperature below the melting point and while under stress.

METHOD FOR STUDYING THE EFFECTS OF TEMPERATURE UPON THE PHYSICAL CONDITION OF ASPHALTS, WAXES, ETC.

By J. A. Capp and F. A. Hull.

The change in physical condition of asphalts, pitches, waxes, and similar substances through a range of temperature is frequently a matter of much importance, especially to those who employ these materials under conditions of stress. A laboratory test method which will give a fairly accurate prediction of what may be expected of a certain material under stated conditions is, therefore, much to be desired. Numerous methods have been devised but practically all of them are limited to the determination of the so-called melting, "dropping" or "flowing" point, or to the penetration of the material by a rod, and tell little or nothing about the change in condition of the material under examination throughout the whole temperature range below the melting point. Necessity led us to experiment with the method outlined in this paper. The method itself was suggested by the Seger cones used in porcelain manufacture.

The detailed instructions for making the test as given to the laboratorians are as follows:

> "Set up the apparatus as indicated in Fig. 1, using cotton seed oil in the oil bath. The test wedges or cones are made with the dimensions indicated on the drawing. It will be necessary to melt the wax and pour it out to the requisite thickness. The wedges can be cut into the required shape after the material has been cooled below room temperature. After forming the wedges bring them, together with the apparatus, to room temperature. Place the wedge in position on the stage as indicated and start heating. The heat should be so regulated that the air bath rises at the rate of 2° C. per minute. Careful attention must be paid to the behavior of the wedges at all times. Reference to

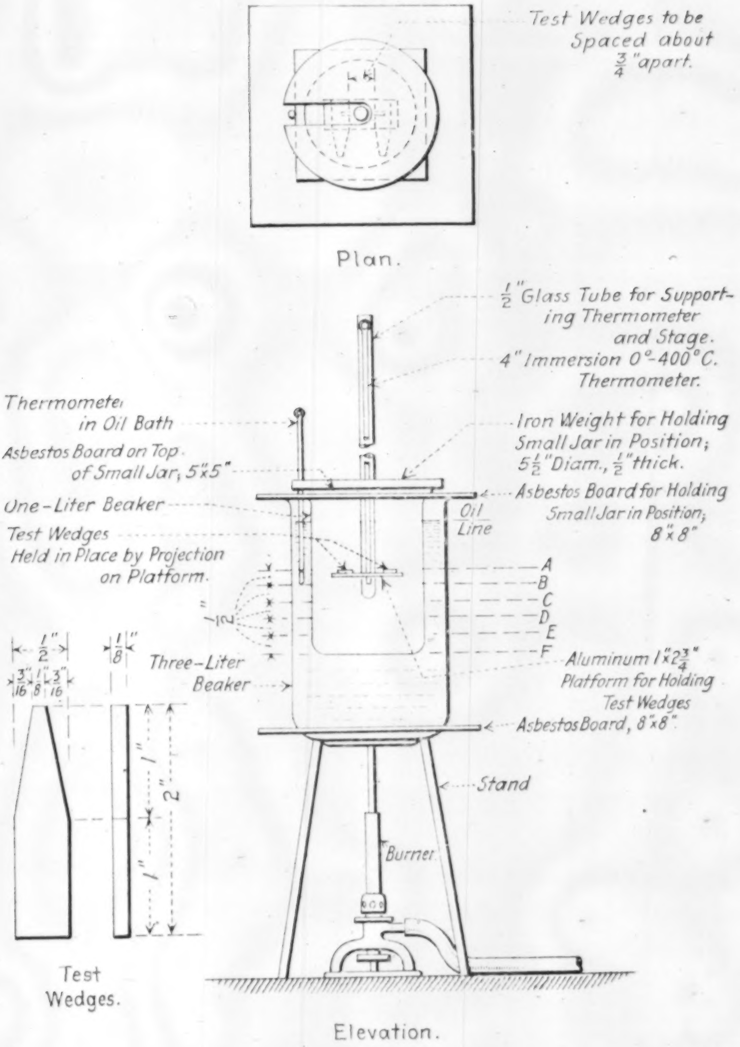
Test Wedges to be Spaced about $\frac{3}{4}$" apart.
Plan.
$\frac{1}{2}$" Glass Tube for Supporting Thermometer and Stage.
4" Immersion 0°-400°C. Thermometer.
Thermometer in Oil Bath
Iron Weight for Holding Small Jar in Position; $5\frac{1}{2}$" Diam., $\frac{1}{2}$" thick.
Asbestos Board on Top of Small Jar, 5"x5"
Asbestos Board for Holding Small Jar in Position; 8"x8"
One-Liter Beaker
Oil Line
Test Wedges Held in Place by Projection on Platform.
A
B
C
D
E
F
$\frac{1}{2}$"
Aluminum 1"x$2\frac{3}{4}$" Platform for Holding Test Wedges
Three-Liter Beaker
Asbestos Board, 8"x8".
Stand
Burner
$\frac{1}{2}$"
$\frac{3}{16}$"
$\frac{1}{8}$"
$\frac{3}{16}$"
$\frac{1}{8}$"
1"
2"
1"
Test Wedges.
Elevation.

FIG. 1.—Test Apparatus and Wedges.

the drawing will show that the wedge on the stage coincides with the line *A* which is marked in a suitable manner on the outside of the beaker. As the temperature rises, in some cases the material will be found to bend gradually so that the tip of the wedge will eventually reach line *B*, while in other cases the material will string out to the bottom of the beaker, which is marked by line *F*. Some

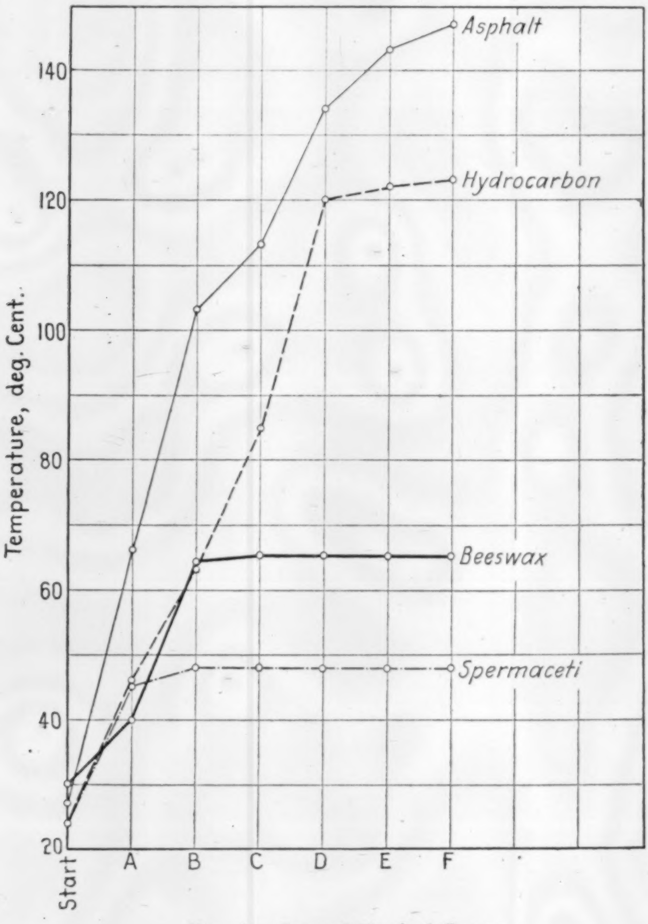

FIG. 2.—Several Typical Curves.

materials will melt and drop off sharply to the bottom of the beaker, or line *F*, without any bending of the wedge. Other materials will be found which will give all the different variations of behavior between the two extremes just noted.

"*Results.*—Note carefully the time and temperature at start and condition of sample, as some materials will bend at room temperature. The time, temperature, condition and manner of bending of the sample should be recorded to show when the tip of the wedge leaves line *A* and when it reaches lines *B*, *C*, *D*, *E* and *F*. Any other general observations on the behavior of the material should be noted and recorded so that the tests will show the behavior of the material over quite a range of temperature. The test is completed when the first material touches line *F*. The results should be plotted on a standard curve sheet, using the temperatures in degrees Centigrade as ordinates and lines *A*, *B*, *C*, *D*, *E* and *F* as abscissas. The curve sheet should also show the other data determined in connection with the sample."

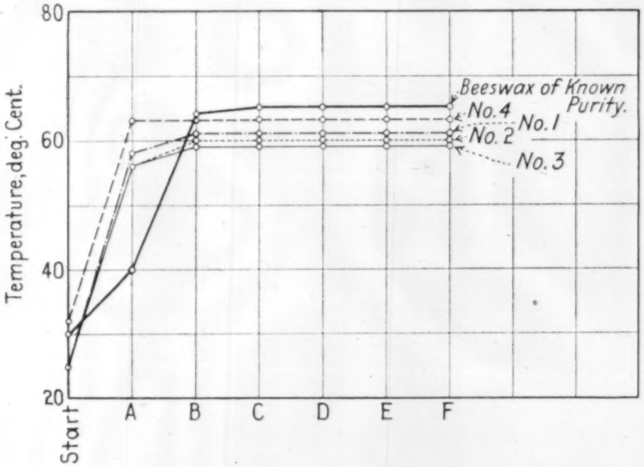

FIG. 3.—Comparison of Some Beeswaxes.

For a period of more than a year, we have made numerous studies of the behavior of a number of materials of an asphaltic, pitchy or waxy nature and have obtained some rather interesting results. In Fig. 2 are plotted curves representing some materials which are comparatively simple in composition and some which are decidedly more complex. It should be noted that the line marked "start" indicates the temperature of the air bath at the time the operation is started. It will be seen that spermaceti, which is largely cetyl palmitate, and at ordinary room temperature is a brittle wax, has a very sharp melting point. It softens at 45° C. just enough to leave line *A* and then drops clear at 48° C. Beeswax, which is a little more complex in composition, has a softening range from 46° C. to 64° C., and then, like spermaceti, it melts sharply and drops off to the bottom of the beaker. The curves for the hydrocarbon and the asphalt, however, behave differently. Instead of melting sharply, both of these compounds bend and string out over a wide range; the hydrocarbon is in a very plastic, almost fluid, condition at 120–123° C., while the asphalt, even at 147° C., is still in a stringy condition, although plastic enough to flow.

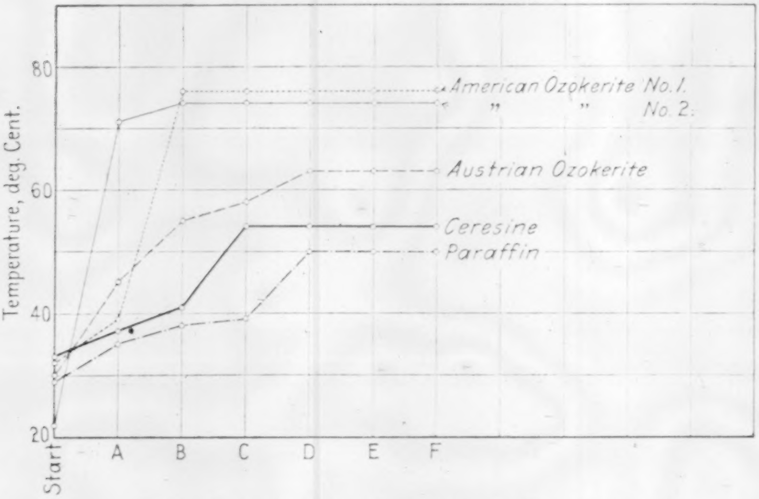

Fig. 4.—Comparison of Some Petroleum Waxes.

In Fig. 3 are plotted the comparative curves of a refined yellow beeswax and four samples of beeswax submitted by a jobber, one of which, No. 4, was claimed to be pure, while the other three samples, Nos. 1, 2 and 3, were admitted to be adulterated. When the tests were made on these samples, it was noticed that sample No. 4 showed a lower curve than the sample of known purity, and it was also noted that the other three samples, admittedly adulterated, gave still lower results. This seemed to indicate that sample No. 4 might also be adulterated.

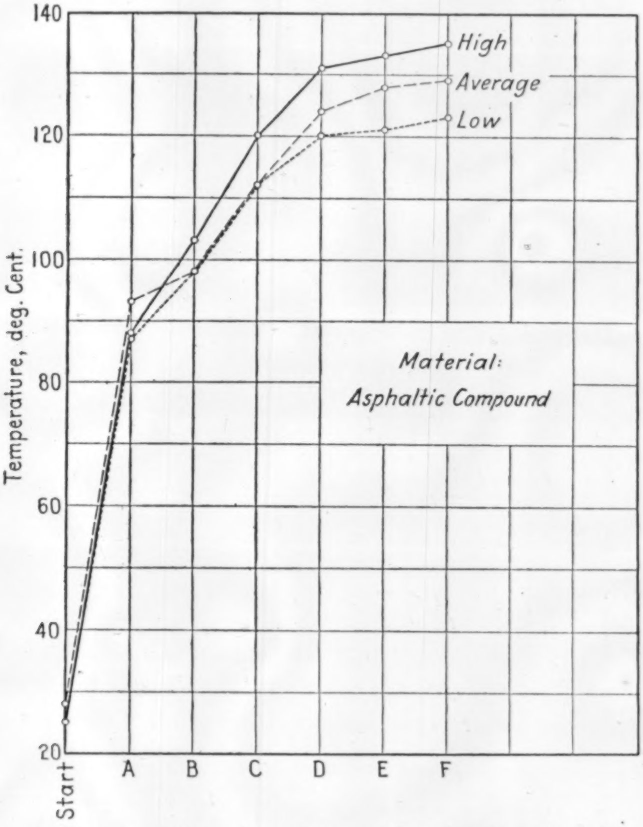

FIG. 5.—Comparison of Several Samples of an Asphaltic Compound.

The results of chemical examination confirmed these indications in every way, showing 5 per cent adulteration in that sample.

In Fig. 4 are plotted comparative curves for refined paraffin, ceresine, Austrian Ozokerite and two samples of so-called American Ozokerite.

In Fig. 5 are plotted first, a curve showing the average of seven different shipments of the same material, and second, the high and low curves from this lot of seven. The material in question is one of the most nearly uniform asphaltic compounds on the market.

The method of testing here proposed is open to the criticism that it is not strictly quantitative, but is really qualitative or comparative, because nothing is definitely measurable except temperatures at which certain approximate physical conditions of the test specimens have been attained. It does, however, yield close comparisons of the behavior of different materials under the same conditions of temperature. During the year or more that it has been in use as a regular method, our ideas concerning the materials used in making up electrical insulations have been considerably modified, because, for the first time, we have been able to study the change in state of the materials and the compounds continuously through temperature changes from ambient temperatures to that of actual melting.

Another method of making the test is to determine the temperature at which the weight of the specimen causes first flexure, and then in another specimen to hold that temperature, or one slightly above or below it, to determine the effect of time. This gives a very fair approximation of what takes place in the rotating parts of electrical apparatus, where the insulation is under centrifugal stress at the operating temperature.

DISCUSSION.

Mr. Herschel.

MR. W. H. HERSCHEL.—I can see how this method is an improvement over those methods which simply determine the melting point, that is, the condition of a substance simply at one temperature; but it seems to me that particularly where substances are used under stress, it would be desirable not only to vary the temperature but also to vary the stress. In this particular case, the stress cannot be varied, being caused simply by the weight of the specimen. I do not see why, in a great many cases, a tube could not be used, as in determining the viscosity of oils. On account of the high viscosity of the substances to be tested, a larger tube and higher pressure would be required.

In some work that was done at the Bureau of Standards by E. C. Bingham on the plasticity of clay, he used such a method. The clay was put in a receptacle which had the tube at the bottom. Pressure was applied to the receptacle, and the material as it flowed through, was collected in a receiver which was connected with the open air so that the clay would always be forced through against atmospheric pressure. The apparatus was in a bath to control the temperature. These materials were so thick that they would not flow through under their own weight; the flow did not begin until a pressure was applied. The amount of flow was obtained by measuring the weight that passed through the tube in a given time, and it was found that there was a distinct difference between a viscous and a plastic substance. With a viscous substance, the volume of flow, or velocity, is proportional to the pressure; but with a plastic substance, the velocity is proportional to an excess pressure, that is, to the pressure in excess of a certain amount required to start the flow. Shown graphically, if the flow is plotted against pressure, a straight line passing through the origin will represent a viscous substance, but a line which intersects the X-axis some distance to the right of the origin, represents a plastic substance. Now, I should like to ask Mr. Capp in what respect

Mr. Herschel. his apparatus serves a purpose which could not be served by a tube, and whether by his method, he ever distinguished between a plastic and a viscous substance.

Mr. Capp. MR. J. A. CAPP.—The only difficulty in making a test through a tube such as has been suggested by Mr. Herschel is that some of these things we have to deal with wet the glass and stick to it, and the condition of friction then between a substance which wets the glass and one which does not wet it introduces too many variables to be taken into account. We gave up the attempt to use any test of that variety. We tried it a long while ago because it was an obviously available method, but it failed simply because of the difference in the way these compounds adhere to or do not adhere to the walls of the tube.

Some of these materials you can call viscous, I suppose. For instance, take a good sized wad of asphalt and let it rest on the table with part projecting over the edge of the table; it will bend down in time even at ordinary room temperatures, and to that extent you can call it a viscous material. Under ordinary conditions of use, however, the material is sufficiently held so that it takes a great deal of the excess pressure spoken of to cause it to move. In practice, the compounds are so applied that they are supported partly mechanically by the shape of the parts in connection with which they are used, and partly by sticking to these parts. Hence, both their support and the friction due to adhesion influence their behavior under stress and temperature. Our test indicates the effects of stress and temperature alone. It requires less stretching of the imagination to apply the simple effects of stress and temperature to a complicated practical case, than would be required to make comparisons between the conditions of test and of practical application where both are complicated by several sorts of unmeasurable variables.

We have not been able to make anything in the nature of a tube test work in practice, nor have we been able to use the penetrometer test; the difficulty of making these tests, and especially of interpreting the results as meaning anything in particular, was such that we gave it up. The proposed test, crude as it is, subject to any criticism you may wish to make of it, actually does give us results which, for the first time, have shown us why we can make successful materials out of certain raw bases and not out of certain others; why those of the second class fail and those of the first do not fail. No other test has shown us that before. **Mr. Capp.**

MR. ROBERT JOB.—I should like to ask Mr. Capp how closely results can be duplicated by different observers; also how long a time the average test requires. **Mr. Job.**

MR. CAPP.—Before we began to use the test at all as a laboratory method, we covered the first point which Mr. Job has mentioned. We set up a battery of four or five of the apparatus and prepared a variety of test pieces from different materials, and then one after another we had several men run the tests on different days, taking good care to see to it that the different men did not know the results obtained by the others; and the curves, which are invariably plotted, were closer than we had any reason or right to expect. I cannot give you the actual figures but the variation was, if anything, lower than the differences that are shown in Fig. 5, which compares the high, average and low points on a series of commercial receipts of a definite material. Those curves as you see are pretty close. The high parts show some differences. At the lower part of the curves there is very little variation. **Mr. Capp.**

The time of test will be perhaps 15 or 20 minutes; never more, and as a rule, less than that.

MR. E. F. HICKS.—Mr. Capp has mentioned having tested other materials than those referred to in his paper, and I should like to ask him whether he has submitted shellac or any other resin to this test; and if so, with what results. **Mr. Hicks.**

Further, with reference to the apparatus illustrated in Fig. 1, what is the best way of attaching the platform to the tube, and why could not the platform be attached directly to the thermometer, thus eliminating the glass tube entirely?

MR. CAPP.—I cannot say whether we have tested such materials as shellac or not; I do not remember. However, we have tested nearly everything that goes through the laboratory; practically anything which can be melted and poured on to a slab to permit the cutting out of the wedge-shape piece can be tested. The second question, with respect to the attachment of the little platform, is only a structural detail; we have **Mr. Capp.**

Mr. Capp. put it on the tube and tied it on. Whether or not it can be tied to the thermometer depends entirely on whether the thermometer is of sufficient diameter. I do not see that there is any particular reason for having a tube except that the tube is big and stiff enough to support the platform. Actually, the thermometer is not put down inside the tube, as shown, but alongside and tied to the supporting tube.

Mr. Voorhees. MR. S. S. VOORHEES.—I should like to ask Mr. Capp if he has established uniformity of temperature in the bath which he uses. We have found several difficulties in attempting to get uniform temperature in a bath heated to approximately these temperatures to determine the loss on heating. In ordinary baths it has been found that there is a marked variation in temperature in different parts of the bath, and an effort was made to develop a method by means of fans and air movement to obtain uniformity of temperature. In the particular instance to which I refer, uniform temperature was obtained by revolving the shelf. May I ask if Mr. Capp has established the fact that the temperature of the bath is uniform?

Mr. Capp. MR. CAPP.—The tests at first were made not with one thermometer in the bath but with a number of them, placed at various parts of the bath, both as to height and as to distance from the center and from the wall of the inner or chamber beaker, and we found that when we controlled the heating flame or whatever means of heat we used so as not to have the temperature of the oil bath widely different from but approximately the same as that of the chamber (the rate of heating therefore is very slow), there is very little difference in temperature, not more than half a degree or so, in different parts of the heating chamber. We have obtained a sufficiently close degree of uniformity for our purpose simply by a slow enough rate of heating.

Mr. Hubbard. MR. PRÉVOST HUBBARD.—I should like to ask Mr. Capp if he considers the "wedge" shape of the specimen any advantage over that of a flat, oblong specimen. I ask this question because of the possible effect of the wedge-shape specimen upon the rate of softening of various materials due to variations in their heat conductivity.

MR. CAPP.—The reason for using a wedge-shape specimen is that the load on the projecting cantilever from its own weight is uniformly distributed. If a test piece in the form of a straight prism were used, the load per unit of area would be less at the end than at the support; that is, it would not be a beam of uniform resistance. Therefore, we use the wedge shape. **Mr. Capp.**

If the rate of heating is slow enough, I do not believe that the question of conductivity enters sufficiently to interfere materially with the results. It would if the rate of heating were too great; but in that case a uniform temperature could not be maintained throughout the test chamber, and that would also vitiate the results.

MR. C. O. HARRINGTON.—Did Mr. Capp use aluminum purposely? **Mr. Harrington.**

MR. CAPP.—Simply on account of the light weight. There is no reason why another material cannot be used. **Mr. Capp.**

DISTRIBUTION OF PRESSURES THROUGH EARTH FILLS.

BY A. T. GOLDBECK.

SUMMARY.

The object of this investigation was to obtain data on the distribution of vertical pressures through earth fills under concentrated loads. The solution of the problem is one of interest to the designer of highway structures. Sand fills were investigated up to a depth of 5 ft. and the pressures under them measured with a special diaphragm cell. From the results of the measurements, the spread of an equivalent uniform load is obtained and is presented in a form readily available for the bridge designer.

DISTRIBUTION OF PRESSURES THROUGH EARTH FILLS.

BY A. T. GOLDBECK.

The present paper gives some results of soil-pressure measurements made with the aid of an apparatus which has already been described before this Society.[1] As the tests progressed certain structural defects developed in the diaphragm cell and changes were made to overcome these weaknesses and render the apparatus thoroughly practicable under working conditions. The principles involved in the manipulation and the general construction remain unchanged. In view of the alteration in structural details, however, it seems desirable to describe the pressure measuring apparatus as it now stands. With this apparatus, the present series of measurements were obtained.

SOIL-PRESSURE MEASURING APPARATUS.

The principles of pressure measurement with the use of this instrument, as already stated in the previous paper, depend upon (1) the equilibration of the soil pressure with air pressure within a small cell buried where the pressure is desired, (2) the detection of the instant of equilibration by the breaking of electrical contact within the cell, (3) the measurement of the air pressure within the cell at the instant of equilibration by the use of a sensitive gage.

A cross-section of the cell as it is used at present is presented in Fig. 1 and its construction is shown there very plainly without further description. When in operation, the movable side of the cell (plan view in Fig. 1) is placed in contact with the earth fill in the direction necessary to give the desired component of pressure, horizontal, vertical, or oblique. The $\frac{1}{8}$-in. pipe containing an insulated wire is led to any convenient accessible place for taking the measurements. The cell may be at any distance from the pressure recording instrument. When taking

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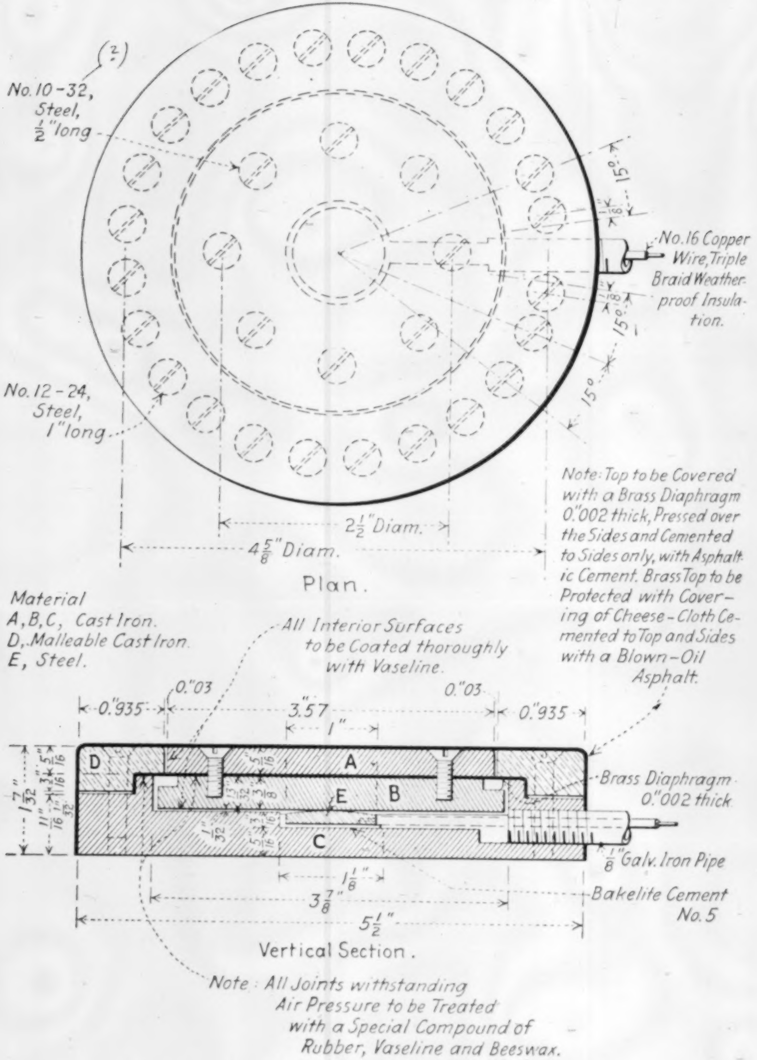
[1] A. T. Goldbeck and E. B. Smith, "An Apparatus for Determining Soil Pressure," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 309 (1916).


(?)
No. 10-32, Steel, ½" long
No. 16 Copper Wire, Triple Braid Weatherproof Insulation.
15°
15°
15°
⅛"
⅛"
No. 12-24, Steel, 1" long
2½" Diam.
4⅝" Diam.
Plan.
Material
A, B, C, Cast Iron.
D, Malleable Cast Iron.
E, Steel.
All Interior Surfaces to be Coated thoroughly with Vaseline.
0."03
0."03
0."935
3."57
0."935
1"
D
A
E
B
C
1 7/32"
11/16"
5/16"
3/16"
5/16"
13/32
3/8
1/32"
1/32
5/16
3/16
Brass Diaphragm 0."002 thick
⅛" Galv. Iron Pipe
1⅛"
3⅞"
5½"
Bakelite Cement No. 5
Vertical Section.

Note: Top to be Covered with a Brass Diaphragm 0."002 thick, Pressed over the Sides and Cemented to Sides only, with Asphaltic Cement. Brass Top to be Protected with Covering of Cheese-Cloth Cemented to Top and Sides with a Blown-Oil Asphalt.

Note: All Joints withstanding Air Pressure to be Treated with a Special Compound of Rubber, Vaseline and Beeswax.

FIG. 1.—Soil-Pressure Measuring Apparatus

a reading, air enters the cell through the pipe and when the air pressure just exceeds the soil pressure, contact is broken between *B* and *E*. This fact is shown immediately by the extinguishing of a small electric light, and the operator at the indicating instrument at once notes the pressure on the pressure gage. This, for practical purposes, equals the soil pressure.

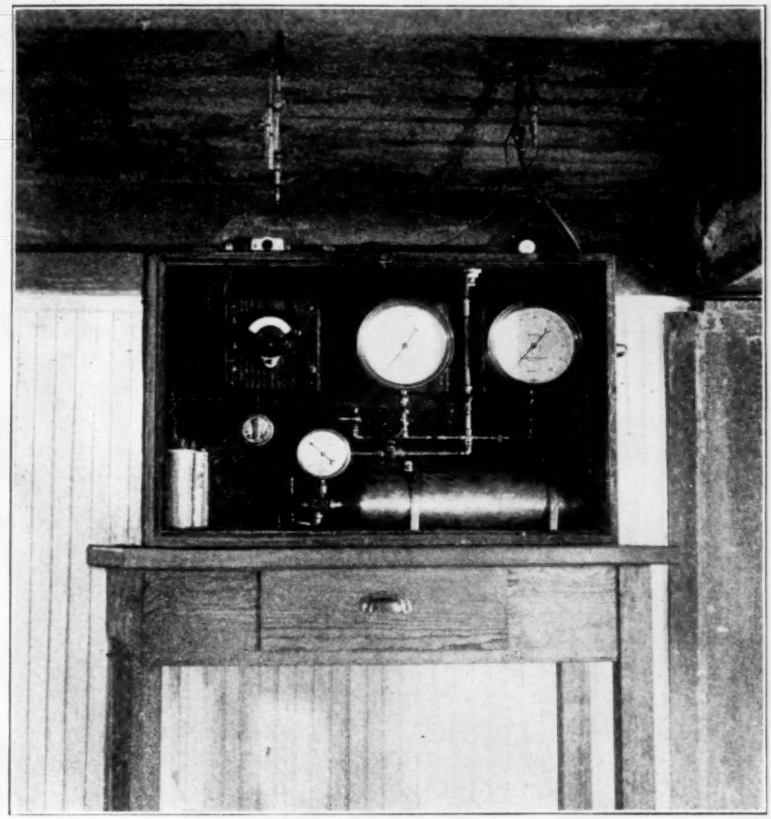


FIG. 2.—Indicating Instrument.

The indicating instrument is shown in Fig. 2. In field measurements this is placed in any desired and convenient position and its air and electrical connections are so arranged that it requires only a few seconds to shift from one cell to another. At the bottom of the containing box is a compressed-air bottle which may be filled with air by means of a hand pump. It is fitted with a rough gage for recording its approximate pressure. Two accurate standard pressure test gages are placed in the line leading to the pressure cell buried in the fill. These appear just above the air bottle. One is graduated from 0 to 30 lb. by increments of 0.2 lb., the other reads up to 100 lb. in increments of 1 lb. A dry cell, a switch, and an electric light are the only electrical equipment required. Necessary valves are placed in the air line to enable the operator properly to control the pressure within the pressure cell. It is possible to so regulate the rate of increase of pressure in the cell that it will not exceed the soil pressure acting on the outside of the cell by more than approximately 0.1 lb. per sq. in. after electrical contact is broken. In measuring the pressure on a cell buried under an earth fill, or at any other place where the pressure is desired, the rubber hose leading from the indicating instrument is connected with the pipe leading to the buried cell and the two wires of the electrical circuit are hung in place. The valve of the air bottle is then opened slightly to permit the air to flow very slowly into the diaphragm cell. The electric switch is turned on, lighting the small electric light. The operator stands ready to manipulate the exhaust valve which allows him to reduce the air pressure in the cell immediately, and he watches the fine pressure gage and the reflection from the light. As soon as the light goes out he reads the gage and at the same time opens the exhaust valve to prevent the air pressure from climbing higher than the soil pressure acting on the buried cell. If this were permitted, future readings might be destroyed, due to the permanent deformation of the soil.

APPLICATION OF DIAPHRAGM CELLS FOR MEASUREMENT OF PRESSURES THROUGH EARTH FILLS.

The measurement of the distribution and intensity of pressure under earth fills due to concentrated loads in only one of many phases of earth pressure measurements on which knowledge is desired by engineers. For carrying on this part of the investigation a special building, having a very heavy, reinforced-concrete floor was designed and constructed by the Office of Public Roads and Rural Engineering at the Arlington Experimental Farm, belonging to the Department of Agriculture. The

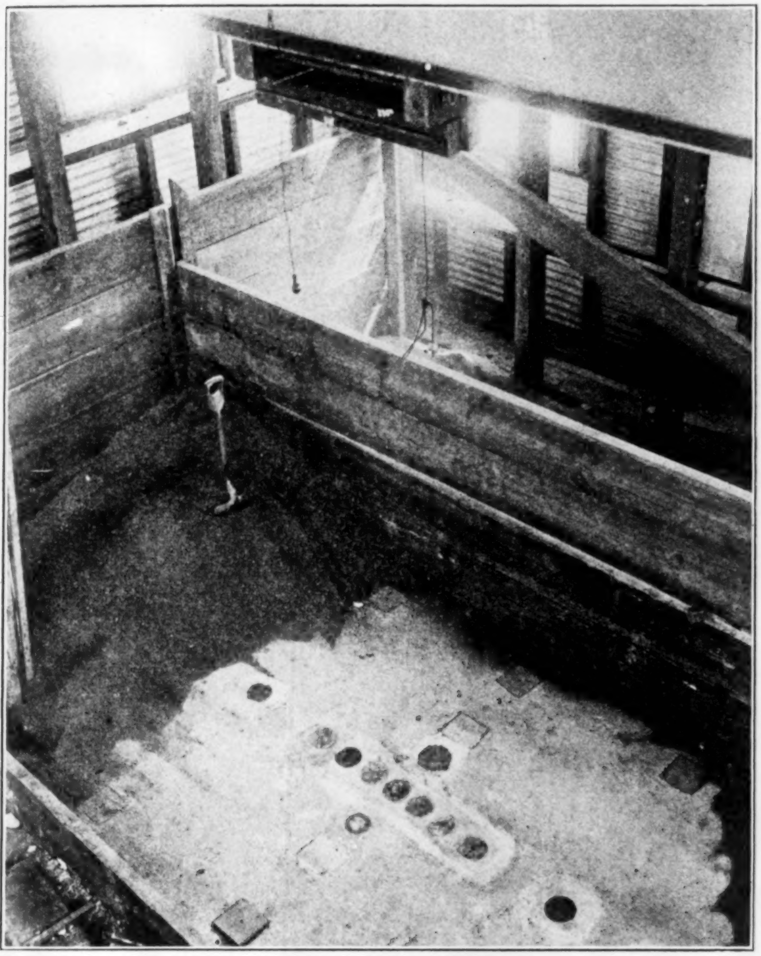


FIG. 3.—Bins for Holding Filling Material, showing Spacing of Measuring Cells.

soil pressure measuring cells were placed in this floor along a straight line with the following spacing: 5½, 5½, 7, 18, 18, 18 in. (see Fig. 3). One cell was also placed 10 in. on each side of the middle on a line at right angles to the long line of cells. The top surfaces of the cells were set flush with the concrete slab and the air pipes led down to the floor below, where the indicating instrument was manipulated. Holes are provided in the floor for mounting other cells should this be necessary.

Bin for Holding Filling Material.—In Fig. 3 is shown the bin for containing the material used in the investigation. It may be made 18 ft. square and 10 ft. high if desired. In the present investigation, however, the bin was cut down to a width of 7½ ft. and a length of 14 ft. for all fills above 2 ft. Up to two ft. the bin was 7½ ft. square. Only in the case of the 5-ft. fill was the size of the bin insufficient to properly take care of the pressure distribution. In this case, the side walls interfered with the transmission of the vertical pressure.

Filling Material and Method of Compacting.—In the present series of tests a coarse concrete sand dredged from the Potomac river was used. It seemed desirable to start the investigation with sand, since it is easy to handle, and moreover it was believed that sand would show the reliability of the apparatus for recording the pressures for which it was designed. The following mechanical analysis best describes the sand used.

MECHANICAL ANALYSIS OF SAND USED IN SOIL-PRESSURE TESTS.

Percentage	passing	¼-in.	Sieve	and	retained	on	No. 10	Sieve	11.0
"	"	No. 10	"	"	"	"	No. 20	"	18.0
"	"	No. 20	"	"	"	"	No. 30	"	25.4
"	"	No. 30	"	"	"	"	No. 40	"	16.4
"	"	No. 40	"	"	"	"	No. 50	"	9.0
"	"	No. 50	"	"	"	"	No. 80	"	12.0
"	"	No. 80	"	"	"	"	No. 100	"	3.2
"	"	No. 100	"	"	"	"	No. 200	"	3.4
"	"	No. 200	"						1.6

The sand was tested in its moist condition, as delivered, and was kept moist by daily sprinkling. There was from 3 to 5 per cent of moisture in the sand, enough so that it would stick together when compressed with the hands. In placing it, layers of about 3 in. in thickness were shoveled into place and heavy wooden tampers were used to compact it. No definite number of blows were given during this compaction, but the sand was simply rammed as hard as it seemed possible by this method.

Device for Applying Load (Fig. 4).—When the proper depth of fill was obtained, the surface was smoothed off and the bearing block very carefully centered over the central measuring cells. Flat circular bearing blocks were used, one 8 in. and the other 13½ in. in diameter. A calibrated I-beam was used for applying the loads. It was provided with proper knife edges and was kept level by means of a screw jack. By placing a 100-lb. weight in various positions on this I-beam, the different loads could be obtained. Extreme care had to be exercised in placing the bearing block and in seeing that it remained level. The slightest disturbance from its position over the central cell or its lack of horizontality would show in the readings of the cells adjacent to the center. Only those readings obtained with the block in as nearly a horizontal position as possible were considered in working up the results.

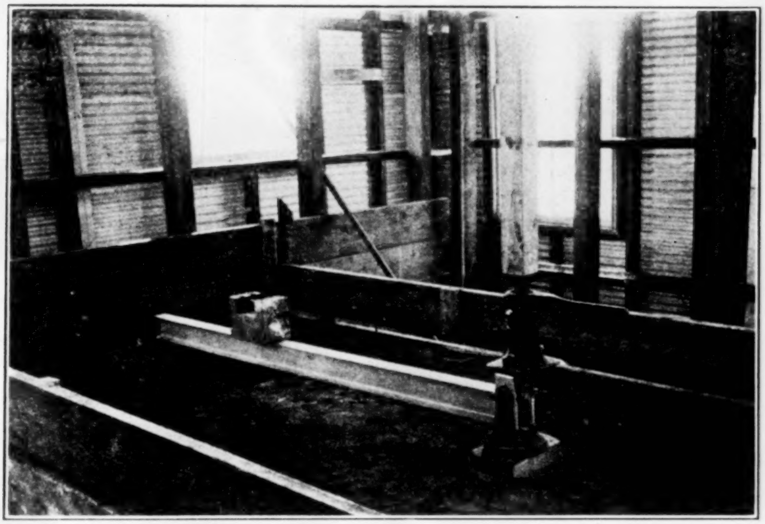


FIG. 4.—Device for Applying Load.

Pressure Readings.—Having placed the sand to the required depth, an initial set of readings was taken showing the pressure exerted by the sand alone. The bearing block and load applying device were then placed and the required load was applied. After each application, pressure indications were taken on each cell. In some cases the process was varied so that the highest loads were taken first and the final reading was made under the weight of the sand alone. Great care had to be exercised in the manipulation of the instrument to see that the air pressure did not run beyond the soil pressure, as this would tend to lower all future readings slightly, due to the displacement of the soil, thus decreasing the pressure on the weighing disk. Under low fills it was found that the curve of readings changed very rapidly only a few inches from the center, and in order to obtain the correct shape of the curve it was found necessary to offset the position of the bearing block one or two inches, thus permitting of taking readings close to the center cell. In this way readings 1 in. off center could be obtained and the shape of the pressure curve very accurately determined.

RESULTS OF TESTS.

The results of the readings are shown in Figs. 5, 6 and 7. The readings due to the weight of sand alone were subtracted from the readings obtained when the fill was loaded. The differences were plotted as ordinates. The abscissas represent the lateral distribution of the pressures. Since a circular bearing block was used, it is to be assumed that the pressures in all lines passing through the center are identical with those measured. The volume of the solid generated by rotating a curve of pressure about its vertical axis, when multiplied by the number of pounds represented by a cubic inch, will equal the reaction. These reactions have been calculated for a number of the applied loads, and are shown in parentheses on the curves. The loads applied are shown outside of the parentheses: theoretically they should be identical, and actually they are nearly equal. When it is remembered that any slight difference in the compaction of the sand in the different radial planes will change the intensity of the pressure exerted in those planes, and when it is remembered that the pressures were measured principally in a single plane and partly in a plane at right angles, the agreement of the

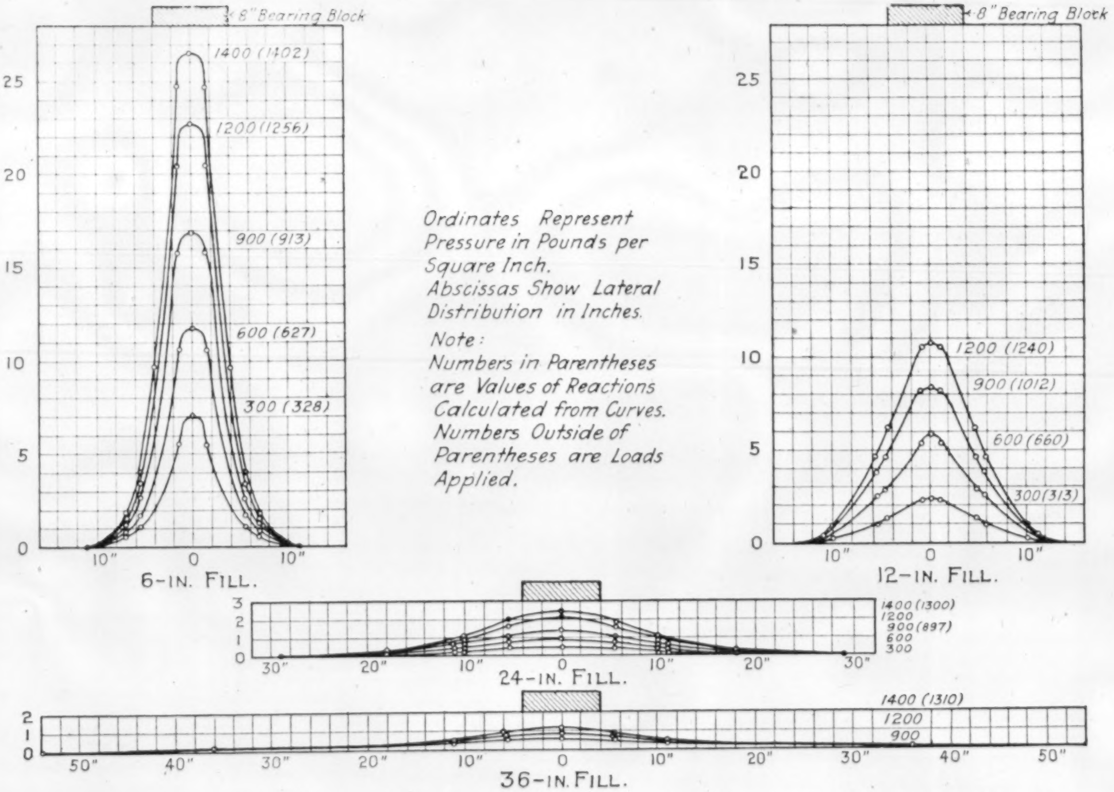
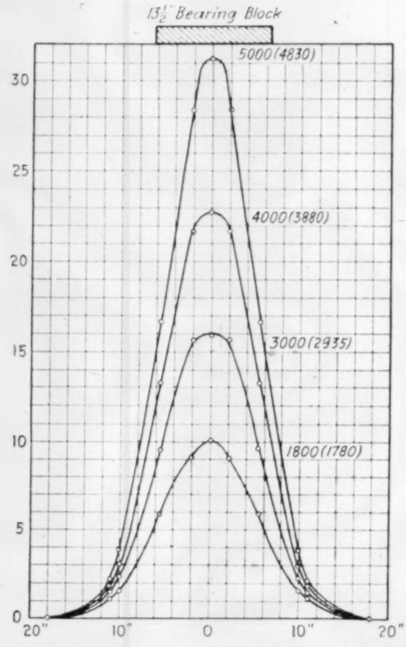
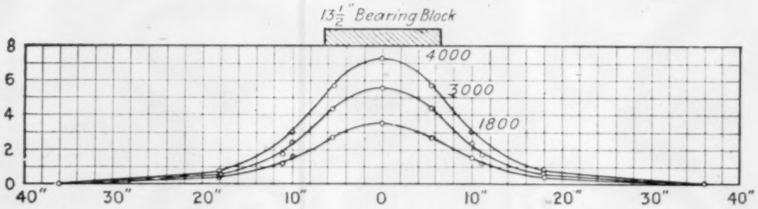


FIG. 5.—Pressure Distribution through Damp Sand Fills. Pressure applied through 8-in. Circular Bearing Block.



12-IN. FILL.

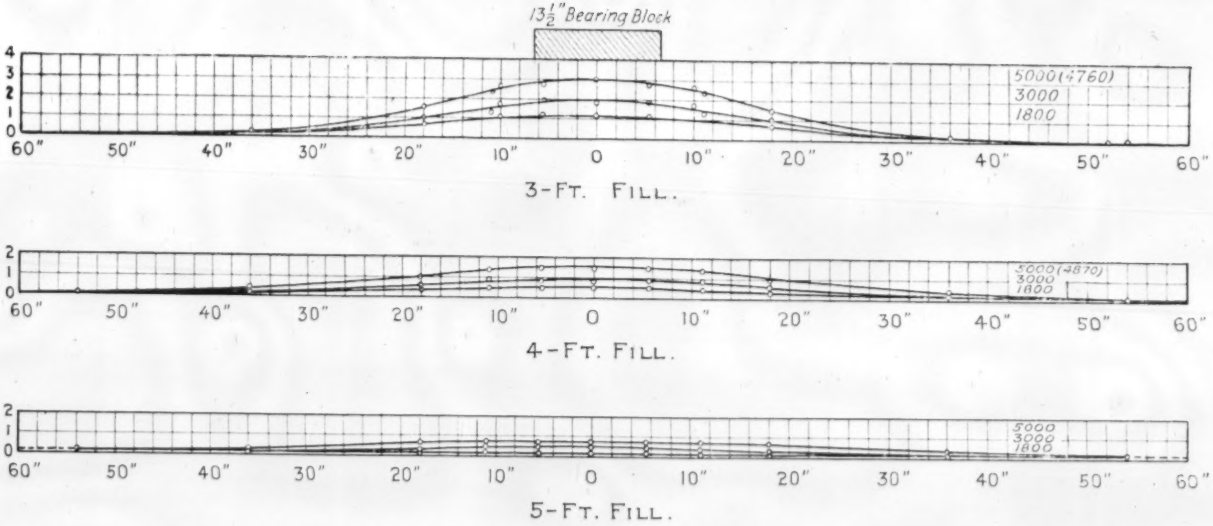


24-IN. FILL.

Ordinates Represent Pressure in Pounds per Square Inch. Abscissas Show Lateral Distribution in Inches.

Note: Numbers in Parentheses are Values of Reactions Calculated from Curves. Numbers Outside of Parentheses are Loads Applied.

FIG. 6 —Pressure Distribution through Damp Sand Fills. Pressure applied through 13½-in. Circular Bearing Block.



Ordinates Represent Pressure in Pounds per Square Inch.
Abscissas Show Lateral Distribution in Inches.

Note: Numbers in Parenthesis are Values of Reactions Calculated from Curves. Numbers Outside of Parenthesis are Loads Applied.

FIG. 7.—Pressure Distribution through Damp Sand Fills. Pressure applied through 13½-in. Circular Bearing Block.

calculated reactions with the applied loads is remarkably close. The reliability of the apparatus for measuring pressures through sand is thus established.

APPLICATION OF RESULTS.

The designing engineer is continually encountering the design of structures subjected to the pressure of earth fills or to pressures transmitted through fills. The present tests were made under conditions which resemble those in a road slab having an earth covering. In the design of such a slab, the engineer is primarily interested in the amount of distribution of a concentrated load furnished by the earth fill. He would like to know the lateral distribution in order that he might figure what width of slab carries the load. He would like to know the distribution forward or backward from the wheel load in order that he might determine the moment about the concentrated load of the distributed pressure load, and thence obtain the bending moment.

It is very common practice at the present time to assume that a wheel load acts over a definite area on top of the fill, that the pressure is transmitted with uniform intensity to the slab, and that it acts over an area determined by an assumed angle of spread from the top of the fill. This assumption of uniform intensity is of course wrong as shown by these pressure measurements, but forms a convenient means of calculation provided the correct intensity and area of pressure are assumed. In order to make use of the curves of pressure for practical calculations of bending moment, it would be well to know the location of the center of gravity of the half volume of the figure generated by revolving the curve about its central axis. The distance from the center of gravity to the center of the curve, multiplied by one-half the total load, is the moment about the wheel load of the distributed pressure to either side of that load. The centers of gravity of these half volumes for the maximum loads applied to the various depths of fill have been calculated, and are shown in Table I.

If for the convenience of the designer, the pressure at the bottom of the fill is considered uniform and acts over a square of area, the sides of each of these areas will be equal to the values in the last column of the preceding table multiplied by four. The sides of the squares of areas thus obtained are somewhat different for the highest and lowest loads applied and may be tabulated as in Table II.

TABLE I.—LOCATION OF CENTER OF GRAVITY OF HALF OF PRESSURE ON SLAB WITH RESPECT TO CENTER OF LOAD.

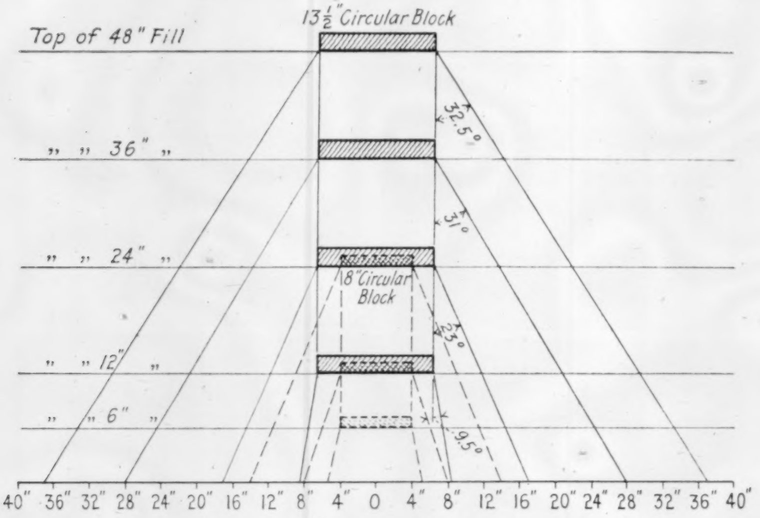
Depth of Fill, in.	8-in. Block.		$13\frac{1}{2}$-in. Block.	
	Load, lb.	Center of Gravity to Center of Load, in.	Load, lb.	Center of Gravity to Center of Load, in.
6	1400	2.6	....	....
12	1200	3.6	1800 5000	4.4 4.2
24	1400	7.0	1800 4000	10.5 8.4
36	1400	16.8	1800 5000	16.0 14.0
48	....	....	5000	18.5

The results in Table II, for the high loads only, are shown graphically in Fig. 8. In calculating the bending moment in a slab, the designer, instead of using the varying intensity of the load, as it is shown to exist in the pressure measurements, may consider the same total pressure applied over a square of area whose side is given in Table II or as shown in Fig. 8.

TABLE II.—EQUIVALENT SQUARES OF AREA.

Depth of Fill, in.	Load on 8-in. Block, lb.	Length of Side of Square of Uniform Pressure Area, in.		Load on $13\frac{1}{2}$-in. Block, lb.
		8-in. Block.	$13\frac{1}{2}$-in. Block.	
6	1400	10.4	....	....
12	1200	14.4	17.6 16.8	1800 5000
24	1400	28.0	42.0 33.6	1800 4000
36	1400	67.2	64.0 56.0	1800 5000
48	....	....	74.0	5000
60	....	....	....	....

It is realized that in order to give complete data on this problem, so that it will be entirely useful to the designing engineer, several other kinds of filling material should be investigated


13½" Circular Block
Top of 48" Fill
" " 36" "
" " 24" "
" " 12" "
" " 6" "
32.5°
31°
23°
9.5°
8" Circular Block
40" 36" 32" 28" 24" 20" 16" 12" 8" 4" 0 4" 8" 12" 16" 20" 24" 28" 32" 36" 40"

Spread of Equivalent Uniform Load Applied over a Square of Area to Produce Moment about Center of Load Equal to that of Actual Load.

FIG. 8.—Equivalent Squares of Area.

and bearing blocks shaped and spaced like wheels and rollers should also be applied to the fill. This will all be undertaken in the investigation still under way, as well as the question of lateral pressures produced by vertical loads.

DISCUSSION.

Mr. R. B. Fehr (*presented in written form and read by the Secretary*).—The paper by Mr. Goldbeck shows that his diaphragm cell apparatus is capable of giving remarkably consistent results in earth-pressure determinations. It seems that at last there has been perfected a small, simple, and compact device which will enable one not only to study the laws of the distribution of earth pressure, but also to obtain important data on earth pressures against completed structures such as retaining walls, culverts, tunnels, subways, etc. Further improvements may possibly be made, for example, along the line of an automatic device for rapidly bringing the air pressure in the cell up to its equilibrium value so as to indicate the pressure due to a moving load, to eliminate the personal error in regulating the air pressure and to prevent an excessive air pressure which would cause errors in future readings; but as far as the reliability and convenience of the apparatus is concerned, it appears that, when used by skilled observers, there are no important defects. **Mr. Fehr.**

Attention may be called to the fact that Mr. Goldbeck's apparatus minimizes the movement of the earth, when taking observations, to a greater degree (as shown by his paper of a year ago) than does the platform scale apparatus employed by the writer in conducting earth pressure tests several years ago at the Pennsylvania State College. In the latter apparatus the movement of the beam was restricted to $\frac{1}{8}$ in., and since the scale ratio was 200, the movement of the platform was 1/1600 in., or practically twice the value of the maximum deflection of the diaphragm cell.[1] In the Pennsylvania State College tests check readings within 5 lb. per sq. ft. were readily obtained, and indicated that a movement of 1/1600 in. was not enough to cause an appreciable error in the observed pressure. Consequently, it is safe to say that the diaphragm cell apparatus with its even smaller deflection gives very reliable results, although it must

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[1] A. T. Goldbeck and E. B. Smith, "An Apparatus for Determining Soil Pressures," *Proceedings*, Am. Soc. Test. Mats., Vol. XVI, Part II, p. 336.

Mr. Fehr. be remembered that greater skill is required in its manipulation than is the case where a platform scale is used, the movement of which can be positively controlled, if necessary, within about 0.0003 in. After all, the best proof of the success of this new apparatus lies in the very remarkable agreement between the reactions and applied loads.

An important advantage of the diaphragm cell apparatus, outside of the matters of compactness and simplicity, is that the distribution of the load is determined by means of *simultaneous* readings and not by the shifting of the load with its attendant errors. For example, the calculated reactions of the Pennsylvania State College tests would be about 50 per cent greater than the applied load if the latter were assumed to be distributed equally in all directions from its point of application. Such an assumption would not be justifiable, however, for the reason that restricting walls cause the pressure to be distributed unequally when the loading area is not in the center of a box whose walls lie within the zone of distributed pressure. Mr. Goldbeck's tests are free from this criticism, except in the cases where the sand fill had a depth of about four feet or more. However, the slight pressures found near the walls of the bin in these cases were not sufficient to cause any appreciable errors in the practical results obtained.

The results of Mr. Goldbeck's tests, when reduced to percentages of unit pressure transmitted, show the same general characteristics as those of the writer. However, the different conditions under which the two tests were run, as for example a measuring area of 10 sq. in. and damp sand in the former as against a measuring area of 144 sq. in. and dry sand in the latter, do not permit of any definite comparison. Nevertheless, it should be noted in the accompanying Fig. 1 that the percentages of transmitted pressure for central loads are higher for shallow fills than those obtained by the writer with his larger measuring area and consequently lower average unit pressure, but not so high as one would be led to expect from the results reported by Prof. M. L. Enger[2] where percentages as high as 300 are shown for a 6-in. depth of sand and loading and weighing areas of $13\frac{1}{2}$ and 4 in. in diameter, respectively. It is interesting to note in the

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[2] *Engineering Record*, January 22, 1916, p. 106.

Mr. Fehr.

accompanying curves that a 20-per-cent transmission of unit pressure for central loads occurs at a depth of 26 in. in Mr. Goldbeck's tests as compared with the writer's value of nearly 25 in.

An important point to note in the tests of this paper is that there was practically no error introduced by the movement of the concrete floor of the bin when the load was applied, because any slight movement that may have occurred was taken care of by the fact that the embedded diaphragm cells moved with the floor.

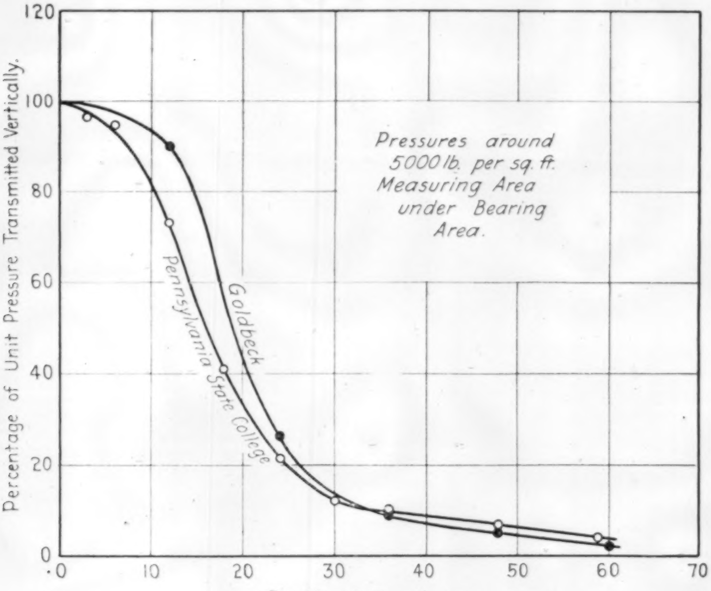

FIG. 1.

In conclusion, the writer desires to point out that, while the apparatus described in the paper is admirably adapted to the determination of *data* on a large scale, it is too difficult to control such conditions as the moisture content, degree of compaction, etc., for a study to be made of the *laws* of the distribution of earth pressure. Consequently, he would suggest that the

Mr. Fehr. diaphragm cell apparatus be applied to a small box having a cross-section of about 4 ft. by 4 ft., and that a study be made of the relation of the bearing area and measuring area, and of the distribution of pressure through various sizes of steel spheres (or lead shot), grain, sand, loam, clay and soil mixtures having various moisture contents and degrees of cohesion, and through different sizes of crushed stone.

Mr. Slater. MR. W. A. SLATER.—As has been noted in the discussion just given, there was an article in *Engineering Record*, January 22, 1916, by Professor Enger, of the University of Illinois, on a problem similar to this. I have noticed in comparing the test data given in this paper with the test data there given, which compares Professor Enger's results with the Pennsylvania State College results, that there is a very great similarity in the form of the diagram, and so far as I have been able to go through the results, I have also found that the variation in results with the depths of measurement between the surface of applying the pressure and the measuring instrument is about the same as those in the Pennsylvania and in the Illinois tests; but I could not check the variation in distribution with the size of the area over which the pressure is applied. I should like to know whether Mr. Goldbeck has made any comparison of his results with the equation given there.

I have noticed that the total earth pressure applied and the reactions computed from the measured pressure at different positions agree very closely, though slight errors are present. In the Illinois tests, which were made with an entirely different kind of apparatus, the errors were greatest with the $13\frac{1}{2}$-in. bearing block and the 6-in. fill. I believe it is true that the same thing was observed in the Pennsylvania test. Mr. Goldbeck's paper does not show the pressure distribution for this combination of size of bearing block and depth of fill. I should like to ask Mr. Goldbeck if his tests cover this case.

Mr. Smith. MR. J. HAMMOND SMITH.—It seems to me that the difference between downward and upward pressures which one notes in the paper may possibly be due to friction on the sides of the bin. In tests on earth pressures on the walls of bins, made at the University of Pittsburgh, this downward tangential pressure on walls was found to exist with all materials tested. Fills

Mr. Smith. of sand, gravel and various other materials were used. This means that in most cases, the resultant pressure on the side wall of a bin, or on a limited area of this wall, is not in a line perpendicular to the surface, but is oblique to the surface: and therefore may be stated as the resultant of a normal component and a tangential component. This may even be true for a large part of the bottom of a bin.

The above argument may be made clearer by the example in which a cubical bin is being filled by means of a chute over its center. The weight of the filling will rest wholly upon the bottom so long as no material comes in contact with the sides; but when the bin is sufficiently filled for material to reach the sides, by rolling, sliding or by leveling, then a considerable part of the weight of the filling will be transmitted to the foundation through the side walls of the bin. An analogous case may be found when a superimposed load rests upon the fill.

I should like to inquire whether the pressure between the upper loading plate and bottom of bin has been investigated. What is the condition, with respect to pressure, between the top loading plate and the diaphrams which measure the pressure at the base?

Mr. Sackett. MR. R. L. SACKETT.—The experimental work at Pennsylvania State College on soil pressure was designed to obtain information concerning the pressure on culverts. In that case the movement of the earth in one direction was restricted, while in the other, at right angles thereto, it was unrestricted. That accounts for certain discrepancies which might appear where other investigators were considering a cone or pyramid with unrestricted movement in all directions. The similarity in results obtained by Mr. Goldbeck at the Office of Public Roads, and by Mr. Fehr at the Pennsylvania State College, are evidence of careful accurate work which warrants confidence in these independent results.

Mr. Goldbeck. MR. A. T. GOLDBECK (*Author's closure by letter*).—As suggested by Mr. Fehr it would be desirable to make improvements in the diaphragm cell apparatus so as to render it capable of recording instantaneous loads for which the present apparatus is not suitable. However, almost all problems in soil pressures involve static pressures alone, so that the apparatus in its present form has a very wide range of applicability.

Mr. Goldbeck. The curves prepared by Mr. Fehr showing a comparison of his Pennsylvania State College results with those obtained by me at the Office of Public Roads and Rural Engineering are interesting. The higher percentages of transmission obtained in the Office of Public Roads and Rural Engineering over those of Mr. Fehr are due to the large size of his weighing area compared with his loading area. As will be pointed out later the percentage of transmission under the center of the loading block increases as the ratio of the loading area to weighing area increases.

The writer quite agrees with Mr. Fehr that it will be desirable to continue these investigations on a somewhat smaller scale in order to better control the character of the filling material, using the large apparatus, however, to check up any laws that might be obtained with the smaller apparatus.

In answer to Mr. Slater relative to the variation in the pressure distribution with the size of area over which the pressure is applied, let me say that since obtaining the results already reported we have made other tests on sand 54 in. and 31 in. in depth using circular loading blocks having areas in square feet as follows: 0.371, 1.02, 1.98, 4.08, and 8.33. We have also investigated loading blocks shaped like truck wheels. With the 54-in. depth of sand, using the flat circular bearing blocks, the percentages of unit transmission directly under the center of the blocks were found to be proportional to their areas. Thus, under 5000-lb. load the following percentages of unit transmission for different sizes of blocks were obtained:

Area of Circular Bearing Block, sq. ft.	PERCENTAGE OF TRANSMISSION AT THE CENTER.	
	From Experiment.	Calculated by Enger's Formula.
0.371	1.6	1.9
1.02	5.3	4.8
1.98	8.7	9.1
4.08	20.7	17.9
8.33	42.5	34.8

A depth of 31 in. has likewise been tested with the above blocks and it is found that the percentages of transmission are a decreasing function of the areas of the blocks. Tests of lower depths of fill are now in progress.

Mr. Goldbeck. A comparison of our results with the equation given by Professor Enger in *Engineering Record*, January 22, 1916, page 107, will be found in the above table which applies to 54 in. of sand only. It seems unlikely that Enger's formula will be capable of general application.

I regret that our series of tests has thus far not included one in which a bearing block of 1 sq. ft. area was used, on a 6-in. fill. It is expected, however, that the above-mentioned range of sizes of blocks will finally be used on this depth of fill. These tests on sand fills should be completed within a few months. As stated by Mr. Smith, it is probable that the small discrepancy between the calculated reactions of the downward loads applied are due to some extent to the friction developed at the sides of the bin. This is particularly true of the higher depths of fill in which case the calculated reactions are somewhat less than the applied loads.

Relative to an investigation of the pressures existing within the fill between the bearing plate and the bottom of the bin, let me say that our investigations have not included these measurements. As stated by Mr. Sackett, the similarity of the curves obtained independently by Mr. Fehr and at the Office of Public Roads is evidence that the results are at least approximately correct. It is recognized by the writer that with the same sand compacted differently or with a different percentage of moisture entirely different curves will be obtained. The results presented are, however, believed to represent what might be called average conditions where a sandy fill over a slab bridge is used. Moreover it is believed that for design purposes, no matter what the filling material is, the results presented will provide a safe working basis. These investigations are being continued by means of laboratory tests as well as from tests of structures in the field. It is hoped that the laboratory tests will furnish a basis for the formulation of laws and that the field tests will furnish evidence of the applicability of these laws.

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